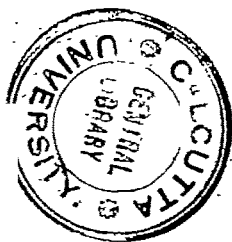


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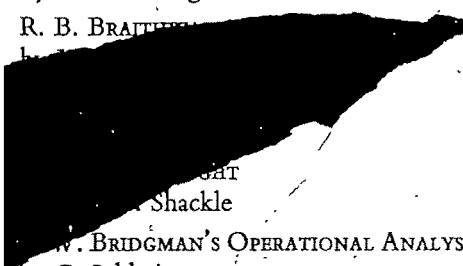
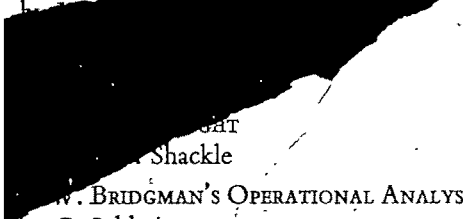
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THE Philosophy of Science Group of the British Society for the History of Science is a body of which the purpose is described by the following statement from its Constitution and Rules :

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THE TWO THESES OF METHODOLOGICAL INDIVIDUALISM *

LEON J. GOLDSTEIN

1 *The Ontological Thesis*

It has become usual in the writings of methodological individualists to suggest that there is but one alternative to their methodological prescription in social science, and this they call 'holism'.¹ Maurice Mandelbaum has sought to counter this by offering a four part classification of types of sociological theories,² thus showing that the alternatives of the individualists is simply an instance of the fallacy of false disjunction. However, it is not at all likely that methodological individualists will find Mandelbaum's paper to the point; it does not deal with the problem that concerns them. I am not at all certain that Watkins, for example, would object to discourse about total societies—the usual meaning of 'holism' and the one with which Mandelbaum deals. He would only insist that such discourse must, at least in principle, be analysable into discourse about individuals and their dispositions, and he does refer approvingly to Ayer's statement that 'the English State . . . is a logical construction out of individual people'.³ We need not be concerned with what Ayer here means by

* Received 31. xii. 57

¹ Cf. J. W. N. Watkins, 'Historical Explanation in the Social Sciences', this *Journal*, 1957, 8, 104-117. In view of the many connotations of 'holism', I deem it regrettable that E. A. Gellner, in his otherwise admirable paper, 'Explanation in History', *Aristotelian Society Supplementary Volume XXX*, 1956, pp. 157-176, continues the practice of using 'holist' for everything non-individualistic. I know of no really suitable term, but I much prefer the more cumbersome 'non-individualistic social science' to 'holism'. (And we shall see that they are by no means synonymous terms.)

² 'Societal Laws', this *Journal*, 1957, 8, 211-224

³ *Language, Truth and Logic*, London, 2nd edn., 1950, p. 63; cited in Watkins, 'Ideal Types and Historical Explanation', in Feigl and Brodbeck, eds., *Readings in the Philosophy of Science*, New York, 1953, pp. 723-743, 730; cf. p. 729 n. 18.

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logical constructions, for I want only to show, since the English state is surely a social whole, that Watkins cannot be said to oppose a social whole. In this example, a holist is one who denies Ayer's assertion, not of necessity one who is interested in the English state.

According to Watkins, 'If methodological individualism means that human beings are the only moving agents in history, and if sociological holism means that some superhuman agents or factors are supposed to be at work in history, then these two alternatives are exhaustive'.¹ He further says, 'The central assumption of the individualistic position . . . is that no social tendency exists which could not be altered *if* the individuals concerned both wanted to alter it and possessed the appropriate information This assumption could also be expressed by saying that no social tendency is somehow imposed on human beings "from above" (or "from below")'.² Holists, it appears, are those who posit non-human entities which in some unexplained way are supposed to determine what happens to men. Human history, on this view, would, paradoxically, not be the history of men and their affairs, but rather of that entity or set of entities that impose themselves upon them. Holists may well be interested in entities that are total, that is, they may conceive their holistic entity to be a unitary sort of thing, and perhaps Hegel's *Weltgeist* is an instance of this. But it is not impossible that holists of more modest pretension could be concerned with entities of a less inclusive nature. Perhaps the 'basic patterns' of the anthropologist Kroeber are such lesser entities,³ and it may be argued that their logic is rather like that of the *logoi spermatikoi* of Hellenistic thought.

If methodological individualism is the thesis that denies the existence of these non-human entities, it is clear that neither Mandelbaum, Gellner, in the paper cited above in a note, nor I, in my earlier paper on this subject,⁴ may be said to have opposed it. Thus, one may wonder what the controversy is all about. The fact of the matter is, however,

¹ Op. cit., p. 106

² Ibid., p. 107; his italics. I agree with the main point of this statement and would add only that included in the 'appropriate information' may well be non-individualistic social laws. Dr Israel Scheffler, of Harvard University, observes that what Watkins says about social tendencies is equally applicable to furniture and machines, any of which could be altered if the individuals concerned both wanted to and had suitable knowledge.

³ A. L. Kroeber, *The Nature of Culture*, Chicago, 1932; cf. chs. 5, 9

⁴ 'The Inadequacy of the Principle of Methodological Individualism', *Philosophy*, 1956, 57, 801-813

TWO THESES OF METHODOLOGICAL INDIVIDUALISM

that under the rubric 'methodological individualism' Watkins has subsumed two positions. The one we have been dealing with is not a methodological position at all. This non-methodological—ontological—version of the principle of methodological individualism is that doctrine which denies the existence of certain alleged entities. The other, more truly methodological thesis is the one which claims that all explanation in social science must, in the end, be reduced to individual dispositions. I shall attempt to deal with what this means in the following section of the paper. For the present I wish to discuss further this ontological thesis. It is not enough, it seems to me, simply to separate the two individualisms and observe that it is quite likely that in the ontological version we discover a point of agreement for most of us who have been interested in this matter. For this, I have two reasons. The first is that the bulk of the rhetorical force of the arguments that are supposed to be in defence of the more methodological of the individualist theses comes from confusing its denial with denial of the ontological thesis, and the fact that for most of us the ontological holist thesis is untenable. The suggestion that non-individualistic social science has evil consequences for freedom and morality has never been made out. Instead, we are supposed to wonder just what can possibly be intended when we talk of freedom and morality in a social world exhaustively determined by non-human entities which impose its history upon man.

My second reason for being unwilling to end discussion of the ontological version is that we are often told that holism is inextricably bound up with another methodological evil, historicism,¹ and here, too, holism and non-individualistic social science are confused. The thesis of historicism is that nothing men do makes a difference; all that will happen happens of necessity given the nature of history, or society, or the dialectic, or the *Weltgeist*. But here 'history', 'society', 'the dialectic' or 'the *Weltgeist*' serve as possible names of a holistic entity, the existence of which both Watkins and I agree in denying. Historicism is, then, an appurtenance of holism, and neither has to do with the claim of some of us that social science must be non-individualistic. Methodological individualists have never shown that to deny the assertion that all explanation in social science must be in terms of

¹ In Popper's sense of historicism, not Mannheim's. By 'historicism' Mannheim means the socio-historical determination of thought, which need not commit him to the view that in history we have the necessary actualisation of what was potential in earlier stages.

LEON J. GOLDSTEIN

individual dispositions entails historicism, and man's consequent helplessness, and I hope to suggest that this is not the case. If they think they have it is because they have not distinguished between their methodological and their non-methodological doctrines.

Whether or not one believes that non-individualism in social science can be defended without the risk of ontological error, it is surely the case that some social scientists have been aware of the distinctions that I have been trying to make above. Thus, Simmel says,

No matter whether we consider the group that exists irrespective of its individual members a fiction or a reality, in order to understand certain facts one must treat it as if it actually did have its own life, and laws, and other characteristics. And if one is to justify the sociological standpoint, it is precisely the difference between these characteristics and those of the individual existence that one must clarify.¹

In the same way, Durkheim remarks,

So there is some superficiality about attacking our conception as scholastic and reproaching it for assigning to social phenomena a foundation in some vital principle or other of a new sort. We refuse to accept that these phenomena have as a substratum the conscience of the individual, we assign them another; that formed by all the individual consciences in union and combination Nothing is more reasonable, then, than this proposition at which such offense has been taken; that a belief or social practice may exist independently of its individual expressions. We clearly did not imply by this that society can exist without individuals, an obvious absurdity we might have been spared having attributed to us . . .²

In sum, there are any number of people who have given thought to this matter who would say with Professor Ginsberg, 'To assign characteristics to groups is by no means the same as to consider them as entities which exist independently of the individuals which compose them'.³

All of these passages show that their authors feel no need of the belief in hypostatic social entities, yet they insist that explanation in social science need not, and perhaps cannot, be individualistic. If methodological individualists want to claim that this is not a consistent set of beliefs, they must show that the rejection of what I refer to as their methodological thesis entails or strongly supports the ontological position we have discovered that Watkins intends by the term 'holism'.

¹ *The Sociology of Georg Simmel*, tr. and ed. by Kurt Wolff, Glencoe, Ill., 1950, p. 16.

² *Suicide*, tr. by Spaulding and Simpson, Glencoe, Ill., 1951, pp. 319 sq.

³ *On the Diversity of Morals*, Melbourne, London, Toronto, 1956, p. 152.

But this has never been done, perhaps never even attempted. And I dare say it never will be so long as methodological individualists do not trouble to distinguish between the two kinds of individualism they support.

• That the rejection of the methodological thesis does not entail or support holism¹ is suggested by Durkheim's referring to the community of consciousness or sentiment as a consequence of individuals living together. There are many such passages in his writings, and his failure to take pains in expressing himself on the point has led his critics to saddle him with belief in something called 'the group mind' or an actually existing collective. We have seen that he denies that this was his intention, but that apart, he is certainly not required to affirm it. It seems to me that a possible construction of Durkheim's words would have him advocating a point of view that may be named 'sociological emergence'. This would claim that, for whatever reasons, when human beings live together, share common experiences, and so forth, there emerge common sentiments and modes of representation² which would never have arisen apart from group life and which cannot be analysed into the bio-psychological characteristics of unsocialised individuals. A view such as this does not require the support of holism. Indeed, it is entirely incompatible with that doctrine as it has been characterised by Watkins. According to Durkheim, collective sentiments emerge out of the social intercourse of human beings, but the holist view is that such sentiments are imposed upon us by non-human entities, the true source of human history. Furthermore, since on the view we are considering what men do does make a difference, there being no emergent sentiments unless there are human beings sharing common experiences, historicism is equally incompatible with it. Presumably, the kinds of sentiments, values, modes of representation, and so on, that emerge depend upon the sorts of experiences that are shared, and man is not treated as if he were caught in the grip of some strange and monstrous being whose necessary development determines the course of history.

¹ That is, the decision to reject the methodological thesis leaves one uncommitted so far as the ontological thesis is concerned, though it seems most reasonable that anyone who agrees with Watkins concerning the former ought to agree concerning the latter as well. Logically, this isn't necessary, for one, being so-minded, could develop a holism in which the holistic entity had about as much to do as a deistic god after the creation.

² Cf. his 'Individual and Collective Representations', in *Sociology and Philosophy*, London, 1953, pp. 1-34.

Sociological emergence is not, to be sure, a sociological theory. It does not in any way explain how it is that social institutions emerge. Nor does it have any specifically testable consequences. But in these respects it is not unlike the methodological thesis of methodological individualism. This, too, explains nothing, nor does it purport to explain anything. No sociological theory need make explicit reference to sociological emergence; its usefulness is of another sort. When methodological individualists assail this or that theory as holistic, when in fact it simply uses concepts that are not reducible to individual dispositions, its defenders have always the possibility of pointing to methodological emergence or some variation of it. That is, since the nature of the criticism levelled against the theory is ontological rather than methodological, sociological emergence offers a way of meeting it. It affirms that social scientists may develop non-individualistic theories without being holists. And it has the further advantage of forcing the methodological individualists to defend their methodological thesis on methodological grounds. If non-individualistic social science does not commit untoward ontological sins, the methodological individualists are required to find better grounds for its rejection. The doctrine that all explanation in social science is ultimately in terms of individual dispositions is not established, indeed, in no way supported, by the untenability of holism.

2 *Individual and Anonymous Dispositions*

There is, as we have seen, a properly methodological thesis included under the rubric 'methodological individualism', namely, the claim that in social science all explanation is to be individualistic. This sometimes means that we are required to carry each explanation back, step by step, until we have transformed sociological explanations into psychological ones.¹ Very often in his writings Watkins talks about *individual dispositions*, and perhaps there is in this mode of speech a reference to the ontological matters discussed above. We are said to be interested in the dispositions of individual human beings and not those of societies or other holistic entities. But this apart, it will be interesting to discover what sorts of thing pass muster, in his view, as individual

¹ 'From this truism I infer the methodological principle which underlies this paper, namely, that the social scientist can continue searching for explanations of a social phenomenon until he has reduced it to psychological terms' (Watkins, *this Journal*, 1952, 3, 28 sq.).

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dispositions. Fortunately, he has himself provided us with a number of examples, and the following quotations may serve as an adequate sample.

What Smith actually showed was that individuals in competitive economic situations are led by nothing but their *personal dispositions* to promote unintentionally the public interest . . .¹

Suppose that it is established that Huguenot traders were relatively prosperous in seventeenth-century France and that this is explained in terms of a wide-spread disposition among them (a disposition for which there is independent evidence) to plough back into their businesses a larger proportion of their profits than was customary among their Catholic competitors ²

Finally, in a letter to me and by way of attempted refutation of my claim³ that the principle of methodological individualism is not adequate to deal with such social scientific work as the theory of kinship nomenclature in G. P. Murdock's *Social Structure*, Watkins writes,

What gives a kinship system its stability and distinctive characteristics is, as you say, certain *rules* about marriage, inheritance, residence, etc., which govern the behaviour of each member to his "relations". But a local rule of inheritance, say, is simply a disposition shared by pretty well everybody in the locality to deal with the property of dead persons in a determinate way. I conclude that there is no difficulty in the idea of an anthropological explanation of the characteristics of a kinship system in terms of disposition and beliefs.

It may be noted that Watkins's closing remarks in the material last quoted indicate that he has misconstrued the point of my paper. I did not there challenge the view that societies or parts of them could be characterised in individualistic terms, though I must confess that I have come more and more to doubt that even this is possible,⁴ but rather the view that sociocultural theory must be individualistic. The problem to which Murdock addresses himself is that of accounting for how it is that systems of kinship develop and change. This is rather different from merely characterising or describing any given such system, and even if one believes that the characterisation may be done in individualistic terms, rather than in non-reducible institutional terms, the status

¹ 'Ideal Types and Historical Explanation', p. 730

² Op. cit., p. 111

³ Goldstein, op. cit., pp. 803-808

⁴ Such characterisations would tend to blur the distinction between the rôles of individuals and the domains of institutions, to use the apt expression of Mr Harold Weisberg. Cf. Mandelbaum, 'Societal Facts', *British Journal of Sociology*, 1955, 6, 305-3

of the sociological laws which govern the development and change of such systems may perhaps require separate determination.

But putting aside this distinction between characterising what I have called the 'synchronic social now', that is, the institution under study during that period of time within which it remains more or less stable,¹ and characterising the laws that govern it, we have to consider what can be meant by calling the dispositions cited in the above passages 'individual dispositions'. It has been made amply clear by Watkins that he wishes to include as individualistic not only the dispositions of specific people about whom we may have factual knowledge, but also the dispositions of anonymous people.² Detailed individualistic explaining may well be an ideal for which to strive, but in most instances it is not a realisable goal, and there seems to be no point in virtually ruling the social sciences out of business by insisting upon it. All of the examples quoted are instances of these so-called anonymous dispositions. They are not only the dispositions of specific individuals with whom the social scientist may be concerned, but if his explanation is adequate we should be able to discover in many other people, concerning whom factual evidence may yet become available, the same characteristics or dispositions described.

I do not doubt that it was individual Huguenots who ploughed back their profits into their respective businesses, but it is not at all obvious that ploughing back—or, for that matter, being a Huguenot—is anything but a social or sociocultural characteristic. Properly to understand this kind of behaviour I am required to know something about the economic conditions of seventeenth-century France, the kinds of business enterprises that flourished, which of them, if any, were especially noted for the concentration in them of Huguenots, and so forth. Presumably I would wish to know other things, not of an economic nature, which bear upon such matters; e.g. if the governments of European countries were wont to interfere in economic affairs, this could be an important bit of relevant information. And thus a more or less full picture of Huguenot economic activities could be developed. But in all this no mention is made of any individual; no concern is felt for the psychological characteristics of anybody. We do not, to be sure, deny, as Watkins thinks we must, that all the time individual human beings are making decisions to buy and sell, to

¹ Op. cit., pp. 809 sq.

² Watkins, 'Methodological Individualism: a Reply', *Philosophy of Science*, 1955, 22, 58-62, p. 62

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manufacture in greater or lesser quantity, and to export or import this or that. But it is not obvious that we need be concerned with them in reconstructing the economic conditions of seventeenth-century France.

- Similar observations may be made about the characterising of the social and kinship system of a people. Specific people are related in specific ways, but the rules governing the determination of these relations are expressed—by the ethnographer even when not fully understood by the people themselves—in ways that do not make reference to individuals or to psychological characteristics. For the most part, people are born into their kinship relationships, and it seems entirely a reversal of actual fact to say that such relations 'are the product of peoples *attitudes* to each other, though these are partly determined by their *beliefs* about their biological relations'.¹ It seems more reasonable to say that for the most part the proper attitudes towards one's various kin are cultivated during the enculturation process.

The point here is that the kinds of dispositions to be found in people of any given type are socially induced dispositions. It seems odd to talk about widely recurring dispositions among Huguenot entrepreneurs and not to wonder about the coincidence of the recurrence in just this group. It was, to be sure, individual Huguenots who successfully competed in the business world of the seventeenth century. But this was presumably because the Huguenot upbringing or enculturation produced people who were adept at this sort of thing. These were people who could operate effectively within the socioeconomic framework of the time.

It seems to me that methodological individualism makes sense only if it can demonstrate the relevance of psychological characteristics in explaining and describing social institutions, and hence I am unable to agree that anonymous dispositions are individualistic. Anonymous dispositions convey no information about anyone in particular, but individuals are individuals in particular. I am not, of course, saying that no individual is disposed to behave in such-and-such way, where 'such-and-such' refers to some anonymous disposition. Bank tellers, to use Mandelbaum's example, are disposed to behave in ways suitable to their occupation, but one cannot explain banking institutions in terms of such dispositions to behave. It is the institutional framework of

¹ From a letter from Watkins to me; it immediately precedes the long sentence quoted in the text above

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banking which fixes the manner deemed suitable for tellers. And when some individual bank teller finally achieves his goal and is promoted to a junior vice presidency, his mode of behaviour changes. But it seems rather odd to say that he has now acquired a new set of personal dispositions and mean thereby that his psychological nature has undergone some change.¹ He has merely stepped into a new rôle and is now prepared to act in ways suitable to his new station in life.

In insisting upon the sociological character of the concepts with which we describe institutions and the social nature of so-called anonymous dispositions, it is not my intention to say that individual people make no difference. Likewise, such insistence need not lead to any undesirable ontological results. There are people and they do make a difference, and yet they are sufficiently similar—at least so we are told by many responsible students of social science²—that laws of social change may be formulated. Not, to be sure, historicistic laws of development, but laws in which the relevant variables are specified and the nature of the change made determinant without recourse to oracular pronouncements that obfuscate more than they clarify. I have suggested elsewhere³ that Murdock's theory of kinship systems is an example of this sort of theorising. This theory proclaims no necessary successions; it requires—as does any theory—only that if it is accepted as true or approximately so, then given the theoretically necessary and sufficient conditions for the development of any kinship system of determinate type, then we may reasonably expect an instance of that type to develop.

Murdock himself actually thinks that behavioural psychology and psychoanalysis are relevant to his theory,⁴ but in actual fact these play no logical rôle in the formulation of the theory or the derivation of its results.⁵ I suspect that his interest in this may be part of a desire to avoid the reification of societies, holism as we now understand it. It is precisely this concern which leads us to sociological emergence, and

¹ Even if it were not odd it would not support the individualistic position inasmuch as it would suggest the dependence of personal dispositions upon non-personal institutional factors.

² This is implicit in a good deal of work that is done in the social sciences and has been explicitly insisted upon by a good many of those who work in those fields, especially by anthropologists. The well known comparative approach of anthropology would seem to make little sense if this were not believed.

³ *Op. cit.*, pp. 803-808

⁴ *Social Structure*, New York, 1949, p. xvi

⁵ Goldstein, *op. cit.*, pp. 807 sq.

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it may well be that if one wanted to determine in specific detail the dynamics of emergence, so to speak, it would be necessary to make use of psychological theory. But I am not able to see that we are therefore required to reduce the social to the psychological any more than we do the psychological to other so-called levels of reality. Nor does it follow that because concern with the actual dynamics of emergence brings us to psychology, that psychological theory has any function at all in the formulation of sociological theories.

The notion that we can talk about the dispositions of anonymous people seems to me somewhat strange. I know what it means to characterise particular people in terms of their particular dispositions. If I am told that John is lazy, I understand that he is disposed to avoid work; if told that he is thrifty, I know him to be a careful man with a dollar (or a pound, as the case may be). These are personal dispositions, though the sort of behaviour characteristic of the expression of such dispositions seems to be cultural, not psychological. But it is one thing to attribute such tendencies to John, quite another to so-called anonymous individuals. I venture to suggest that wherever Watkins talks about anonymous dispositions or the dispositions of anonymous individuals he is simply attempting to talk about non-individual characteristics of societies or part of societies, or of socially induced ways of behaving, without being explicit about it. Anonymous individuals are referred to as a way of avoiding holism, but we have seen that this may be done in other ways. There are specific individuals and characteristics of social phenomena; each of these raises theoretical questions of its own, and it is the business of psychology and social science to deal respectively with them. But there is no science of the anonymous. What we have are not the characteristic dispositions of people we don't know, but the social behaviour of people in given situations quite apart from their personal dispositions.

THEORIES, DICTIONARIES, AND OBSERVATION ★

MARY B. HESSE

I

ACCORDING to a widely accepted account of the logic of scientific theories, statements of a theory may not be directly testable, since they may mention concepts which are not empirically observed, but it is necessary that some of the logical consequences of the theory shall be interpretable in terms of observation statements which are directly testable, and that no such observation statements shall have been in fact falsified. A distinction is commonly made between direct observation and theoretical inference in such a way as to make it appear that observation statements constitute a basic empirical language which is independent of all scientific theory, and which can thus function as the ultimate court of appeal for the truth of the theory. I shall first examine a few examples of this distinction, and then try to show that if the observation statements are to be tests of the theory, the distinction cannot be maintained.

(i) In the account of N. R. Campbell,¹ the empirical language consists of statements of laws of nature which assert uniform association or functional relationship between 'concepts', which, although not 'simple judgements of sensation' (p. 45), because their definition often depends on the truth of some other law of nature, nevertheless seem to be directly measurable quantities, since Campbell's examples include 'length, weight, potential difference, electrical current, . . . temperature' (p. 105), and not, for example, 'volume of space or the diameter of gas molecules'. 'The experiment' (p. 130), proved (p. 131).

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Hypotheses, on the other hand, according to Campbell, have a different status from laws, since they have not this direct relation to experiment, but they are connected to laws by means of a 'dictionary' (p. 122), which 'interprets' hypothetical ideas into concepts, so that the laws allegedly relating these can be tested.

(ii) F. P. Ramsey,¹ following Campbell, speaks of a *primary system* containing all the terms and propositions of the universe of discourse in question (in his examples these are sense-data), and a *secondary*, or theoretical, system, related to the primary system by means of a dictionary which defines 'the functions of the primary system in terms of those of the secondary system' (p. 215). Many propositions of the secondary system will not have 'any *direct meaning*' (p. 235, Ramsey's italics).

(iii) Professor Ayer² speaks of 'observation-statements' which record 'an actual or possible observation' (p. 11), and regards a statement as *directly verifiable* if it is an observation statement, or 'is such that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone' (p. 13). Directly verifiable statements are distinguished from those that are *indirectly verifiable* but which appear in scientific theories although they do not designate anything observable. A characteristic of indirectly verifiable statements in science is that they are connected with directly verifiable statements by means of a dictionary which 'can be regarded as analytic'.

(iv) Professor Braithwaite³ distinguishes between 'propositions about observable entities', for example 'flashes of light or pointer-readings of a measuring instrument' and those containing *theoretical concepts*: 'fields of force, wave-functions, electrons' (p. 51). Direct meaning is given to the first kind of proposition, indirect meaning to the second.

(v) Professor Woodger⁴ divides biological statements into (1) observation records and (2) theoretical statements. The latter all go beyond what can be directly observed and therefore cannot be verified, but their consequences are testable, and it is upon this consequence-structure that they depend. 'It is with the help of this relation [of consequence] that the testing of hypotheses is possible, and this is

¹ *The Foundations of Mathematics*, London, 1931

² *Language, Truth and Logic*, 2nd ed., London, 1946

³ *Scientific Explanation*, Cambridge, 1953

⁴ *Biology and Language*, Cambridge, 1952, p. 12

independent of the meaning of the descriptive signs which occur in the *high-level* hypotheses. For this reason it is quite unnecessary that the descriptive signs in such statements should have "meaning" in the sense that we should be able to represent them as objects which we can see or imagine' (p. 58).

The view of science implied by these five writers agrees in the following points:

(a) There is some set of statements whose truth or falsity is known directly by observation, although it is not necessarily known indubitably. It is assumed that what these statements are is sufficiently indicated by giving examples and counter-examples.

In order to avoid further confusion of terminology, I shall call these statements *phenomenal*, a word which is neutral as between Campbell's laws of nature asserting relationships between 'concepts', Ramsey's 'primary system', Ayer's 'observation-statements', Braithwaite's 'propositions about observable entities', and Woodger's 'observation records'. I wish to avoid use of the word 'observable' at this stage for reasons which will be apparent later, and the word 'phenomena' has the merit of having been used by Newton and by Kant in senses not too far removed from what is here intended. The word is not, however, to be taken as carrying any overtones of phenomenalism, if by that is meant a doctrine according to which all we directly know are sense-data. Phenomenal statements are intended to cover all ordinary indicative sentences descriptive of physical objects, including some general sentences.

(b) The five views agree that there are statements in science which do not fall into the phenomenal class. I shall call these *theoretical statements*.

(c) The views further agree that the meaning and criteria of truth of phenomenal statements are independent of those of theoretical statements.

(d) It is also said that the meaning of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements.¹

I shall call the view of science expressed in these four points the 'dictionary theory'. I suggest that the motive for its adoption was twofold: firstly, to contrast the clarity and certainty of empirical tests with the tentative nature of theories, and secondly to show nevertheless how theories could be unambiguously tested and hence

¹ Woodger does not use the word 'dictionary', but the idea is implied by his discussion of the meaning of hypotheses quoted above.

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given meaning by experiment, and so distinguished from speculative metaphysics for which no such tests were available. What I wish to show here is that if phenomenal statements are to be tests of theories, then their meaning cannot be entirely independent of that of the theories, and that consequently the function of the dictionary has been misconceived in these accounts of the relation between theory and experiment. Then I shall discuss the use of the word 'observable' and its cognates in specifically scientific contexts.

2

It is necessary first to define phenomenal statements a little more closely, since it is generally agreed that they cannot be characterised by their certainty or incorrigibility. It is easy to see that they cannot be *logically* certain, for there is no necessary logical relation between the occurrence of a single public event and the written or spoken report of it, much less between a series of allegedly similar events and the general phenomenal statement which asserts that under these given circumstances, such and such always occurs. The phenomenal statements of science are almost always general in this way: they do not describe what happened on a particular occasion to a particular observer, but what always happens and will happen on sufficiently similar occasions to all normal observers. Such statements cannot be claimed to be incorrigible, but they are characterised by a high degree of *empirical* certainty (for the possibility of mistakes is minimised by experimental techniques), and of invariance with respect to repeated tests and different observers.

What is not so often noticed, however, is that the claim that phenomenal statements are unambiguous cannot be maintained either. This claim is implied in the assertion that the meaning of the statements is independent of any theories, and it relies upon the existence of a phenomenal language which involves no technical terms and which is understood by everyone who speaks the natural language in question, and in which the phenomena upon which scientific theory ultimately rests can be unambiguously described. Now it is perfectly true that there is such a language, containing only the common-sense descriptions of ordinary objects and processes, and also, if necessary, descriptions of what come near to sense-data in dealing with unfamiliar objects and processes encountered in scientific observation. All scientific observations can be described in such terms, mentioning apparatus whose

specification cannot involve any knowledge of theory, since it has in any case to be understood in the workshop, and describing coincidences of pointers with scales, and clicks, flashes, bands of coloured light, etc. No doubt it is impossible to make precise this notion of being commonly understood in the language, for what is commonly understood among one group of people may not be so among others, but I shall assume, as do most of the exponents of the dictionary theory, that it is possible to recognise a statement as being phenomenal in this sense, and that if any alleged phenomenal statement is challenged because it contains some technical phrase such as 'current in the wire', it is always possible to find an acceptable translation into phenomenal terms, such as 'the position of the pointer on that scale'.

Phenomenal statements, then, differ from ordinary empirical descriptions only in having the characteristics of certainty, publicity, and repeatability to a higher degree than is usually possible or necessary. Normally descriptions of experiments are not given in this form because to do so would be tedious and unnecessary, but if necessary, for instance when radical revisions of a theory are in progress, most descriptions could in principle be reduced to phenomenal statements as here defined. The dictionary theory asserts, however, that such phenomenal statements function as tests of theoretical statements in the sense of being derivable from the latter, and being determined as to their truth or falsity by empirical considerations which are independent of theory. I now wish to show that if phenomenal statements are to be tests of a theory, they cannot be independent of the theory as here suggested. The conditions given for phenomenal statements are not sufficient for them to have scientific significance, and if they are to have such significance, there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped. Physics can be reduced to phenomenal statements if these connections of meaning are ignored, but such reduction does not give a full account of the observational basis of physics, for it cannot explain why just these statements are chosen and not others.

3

A phenomenal statement can never test a theory alone, but only a theory together with its dictionary interpretation into this particular phenomenal statement. Consider the stick apparently bent when

immersed in water. The situation can be described in detail in a phenomenal statement using only words universally understood: 'surface', 'sun', 'air', 'water of given salinity' (this last one can be further unpacked), and so on. Suppose we set up a theoretical deductive scheme mentioning light rays and Snell's law, and derive from it an angular measure. With the appropriate interpretations 'source of light' into 'sun', 'media of different refractive index', into 'air and water', and so on, we can identify this theoretical angle with our angle measurement, and assert that the phenomenal statement confirms the theory of refraction together with this interpretation.

But how do we know that this is an interpretation which is relevant to this theory? If in the equation expressing Snell's law, $\sin \alpha / \sin \beta = \mu$, α and β were simply undetermined mathematical symbols, they might be interpreted in an indefinite number of different ways, some of which might be shown to be true by observation. They might for example be the angles between the Pole Star and Mars and Venus respectively at midnight on certain given dates; why would not this be a confirmation of the formalism we have mistakenly called the wave theory of light? Conversely, why can we not invent any theory at all which leads deductively to the equation $\sin \theta / \sin \phi = \mu$, and interpret θ and ϕ as the angles of refraction, and take our refraction experiment as confirmation of this theory?

The answer is of course that the possible interpretations of α and β are already circumscribed by the theory, which itself talks about light rays and media of different refractive index, and that we already know the kind of observation which will be relevant to this theory. If the theory is to be acceptable the equation $\sin \alpha / \sin \beta = \mu$ must also be interpretable into other relevant observations besides that of the bent stick. Suppose we take a burning glass and use it to burn a piece of paper. The concentration of heat at a given point will only be a confirmation of the theory of refraction used to explain the bent stick if it is assumed that 'media of different refractive index' may be interpreted into 'air and glass' as well as 'air and water', 'interface' may be 'curved surface' as well as 'horizontal surface', and 'radiation' may be 'heat' as well as 'light'. That these are relevant interpretations cannot be given in the phenomenal statements alone, because by definition these cannot contain theoretical overtones. It may be objected that no theory is required to see that the phenomena of bent sticks and burning glasses are connected, and that it is obvious to common-sense that both need accounting for in terms of the same

theory. This may be true, although it clearly depends on the extent to which 'common-sense' has been permeated by scientific notions. But there are other examples in which it is certainly not true. To a layman entering an atomic physics laboratory it would not be at all obvious that certain pairs of scintillations over here are connected with the presence of a nuclear reactor over there, and that they confirm a theory which explains why certain other scintillations near the reactor have the character they have. But to a physicist they count as the detection of neutrinos whose presence was suspected because of a loss of energy in certain decay processes in the reactor. The mere report of scintillations expressed in a phenomenal statement cannot proclaim its own relevance to this theory, nor its connection with phenomena on the other side of the laboratory; the observation becomes a confirmation of the theory only when it is interpreted into language about fundamental particles, and this can certainly not be done in phenomenal statements as they have been defined.

It might be suggested at this point that all the inferences involved in interpreting the observation as a test of the theory could be broken down into phenomenal statements together with such elementary links between them as that involved in seeing that two simple optical phenomena are connected, as in the first example. But this is just what is done when the neutrino theory is *taught*, and it is a process which involves teaching the whole of physics. If all this is necessary before a phenomenal statement can be said to be a test of the theory, then it certainly cannot be maintained that the testing function of the statement is independent of theory.

There is a view of theories which might seem to avoid the objections here brought against the simple dictionary theory, that is, that the function of phenomenal statements is not to *test* theories, but merely to determine their *scope*.¹ Thus it might be said that the burning-glass experiment by itself is not a test of the wave theory of light, because whatever the result of the experiment, the theory could be saved by appropriate, and perhaps very limited, interpretations of its symbols. Similarly if the expected scintillations were not observed, this would show only that the scope of the neutrino theory did not extend to these particular experiments. But it would be very queer to say this. Burning glasses and these kinds of scintillation are just what the theories respectively are about, and if the obvious interpretations, which are determined within the theory, do not lead to confirmatory observations,

¹ Cf. S. Toulmin, *The Philosophy of Science*, London, 1953, p. 63

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then some further theoretical explanation of this fact is required, or else the theory is abandoned. There are of course cases where it is the scope of a theory that has to be determined by observation, for example Boyle's law is confirmed only over a certain range of temperature and pressure, but then we look for a more general theory to explain the deviations from Boyle's law, and this theory has in turn to be tested. It is not profitable to confuse this kind of example with the others where a theory is properly said to be tested.

Or again, it may be suggested that scientific theories are just abstract correlations of some phenomenal statements or other, no matter what these phenomenal statements are; that we can, as it were, parcel together any collection of phenomenal statements from many diverse areas of experience, and that so long as we can find a correlating theory, this will be a satisfactory piece of scientific explanation. This is not as implausible as it sounds, for, as Professor Dingle has pointed out, something like it happened in seventeenth-century physics when study of Aristotelian substances with diverse qualities was abandoned in favour of the study of the primary qualities common to all substances. Phenomena involving mass and motion, however diverse in other ways, were parcelled together under the theory of mechanics, while complex individual substances were divided into various aspects and became subject to many different kinds of theory. That many different parcellings of phenomena for the purposes of theoretical explanation are conceivable does not however show that these parcellings can be quite arbitrary. The fact that one can, as in this example, describe the principles upon which the new parcelling was done, namely that primary qualities were distinguished from secondary, shows that it was *not* arbitrary. That it can never be quite arbitrary and remain scientific is sufficiently shown by considering that a theory developed *ad hoc* as a correlation of a completely diverse collection of phenomenal statements could never be predictive, for what would decide what kinds of phenomena the theory was supposed to make predictions about? One could only do this by reference to new phenomena; freedom;

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If this is the case, is there any justification at all for the two-tiered structure of statements described by the dictionary theory? It is already admitted by most writers that the meaning of phenomenal statements is contextual, and if the context includes certain theoretical ideas, introduced by the use of even the most simple technical words in phenomenal statements, must one not characterise the situation quite differently, and say that theoretical language is *richer* than phenomenal language and never even in principle translatable into it?

To interpret an experiment directly in theoretical terms so that it can be a test of the theory is always to say more than the corresponding phenomenal statements would say, because such interpretation carries with it natural expectations about possible but so far unobserved behaviour which the scientist has to *learn*, just as the child learns the contextual overtones of ordinary language. Suppose an observation is reported in the following terms: 'if in a Wilson cloud-experiment the quantum of scattered X-rays produces a photo-electron in the chamber, then a line drawn from the beginning of the recoil track to the beginning of the track of the photo-electron gives the direction of the quantum after scattering. It was therefore possible to test the truth of the equation

$$\left(1 + \frac{h\nu}{mc^2}\right) \tan \phi/2 = \cot \theta$$

which connects the directions of the scattered quantum and the recoil electron.'¹ This cannot be translated without remainder into a phenomenal description of the cloud-chamber and an apparatus producing X-rays, together with the statement that there are two finite non-intersecting white lines and the observer has drawn a third line joining one end of one to one end of the other, and measured the angles between the lines. This is the whole phenomenal account of the experiment, and clearly it means nothing and could test nothing, because it does not state its relevance to anything. θ and ϕ as measured on the photograph may satisfy the equation, but they cannot be known to be the θ and ϕ of the theory from which the equation is derived. The equation is a theoretical statement, and its terms

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theoretical and phenomenal languages is not the relation between simple sentences in English and in French, but the translation of poetry into pedestrian prose; or, to look at it the other way round, the phenomenal description of an experiment has a relation to the scientist's theoretical description which is similar to that between Holingshed and Shakespeare. The hierarchy of statements in science is like the gradation from prose to poetry in being characterised by varying degrees of imagination, but it is also unlike in being correlated approximately with degrees of factual certainty.

There is, however, a place where talk about dictionaries is in order, and this can be explained very briefly. It frequently happens that uninterpreted mathematical calculi are used to perform the deductions which are required by a theory. This is clearly explained by Braithwaite (op. cit., Chaps. II and III). Where uninterpreted mathematical expressions are used, as for example, $x'' + n^2x = 0$, in order to facilitate deductive arguments by calling upon the apparatus of pure mathematics, it is necessary that somewhere a connection should be made between the signs appearing in these expressions and the scientific language. Here the process of translation is properly said to be carried out according to a dictionary. But the point which has been confused by exponents of the dictionary theory is that the *interpretation need not be into phenomenal statements*. The above equation is adequately interpreted for use in part of atomic physics by the following entries in the dictionary:

x is the amplitude and $2\pi/n$ the wave-length of stationary waves representing a single electron moving in a finite straight line in a field of constant potential.

It goes without saying that the concepts in this definition are not phenomenal.

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We have now reached the point at which most methodological discussions *by physicists* begin, and indeed if they are not familiar with current philosophical writings on scientific method, they tend to regard such remarks as those above as trivial and obvious. But we are now in a better position to understand a confusion perpetuated by some writers in physics about the notion of 'unobservable entities'. What has been shown so far is that two different classifications of scientific statements have been confused by the dictionary theory:

(i) The distinction between theoretical and phenomenal statements, which does not call for *translation* but for *interpretation* of observations

at different interpretive levels involving more or less reference to theoretical concepts.

(ii) The distinction between formal and interpreted statements, in connection with which it is appropriate to speak of translation by means of a dictionary, but where the interpretation may be into theoretical rather than phenomenal terms.

To these two classifications must now be added a third, which corresponds to the use by physicists of the words 'observable', 'factual', 'objective', 'physically significant', as opposed to such phrases as 'mental conceptions', 'fictitious entities', and 'mathematical equipment'. In the philosophy of science since Newton the distinction implied by the use of such words as these has been invoked on various occasions and the pattern of thought is usually the same. Some piece of physics has raised difficulties either by contradicting common-sense conceptions of the world (for example Newton's action-at-a-distance) or by appearing self-contradictory (incompatible properties of the material aether or of fundamental particles). Philosophers or physicists of an empiricist persuasion have then asserted that the difficulty can be eliminated by regarding scientific theories which go beyond immediate experience as mere mathematical constructions, tools for correlating and predicting the results of possible experiments, but not as descriptions of physical reality. It is more comfortable to regard ourselves as using different tools for different purposes than to believe that we have found self-contradictions in reality. This was the line taken by Berkeley and Kant in relation to Newtonian science, and by Mach, Pearson, and Duhem in relation to nineteenth-century physics; it was generally, but not necessarily, associated with a phenomenalist theory of perception, and it usually blurred the distinction between the theoretical-phenomenal and formal-interpreted classifications of scientific statements. Before the twentieth century however, such epistemological discussion did not appear in physics itself, but only in commentaries upon it. The apparent contradictions within physics were resolved by learning to accept different kinds of model — central forces acting at a distance instead of Cartesian mechanism, or Maxwell's equations instead of a mechanical aether. But in twentieth-century physics, the word 'observable' has become a technical term of quantum theory, and physicists engage in apparent epistemological discussion *while doing physics*, so that the misleading impression is given that physics itself has come down on one side of the epistemological fence, and that this technical discussion is the same

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as that carried on by traditional phenomenologists and empiricists. Support is given to this impression when philosophers of science are found equating 'unobservable entities' with 'theoretical (as opposed to phenomenal) concepts'. We have already seen that it is misleading to speak of all theoretical concepts as unobservable, because all observation for the purpose of testing a theory involves some interpretation, and the interpretive report of an experiment may be given in theoretical terms. It is proper (and usual), for example, to say that the scattering of electrons by gas particles is *observed*, even though the phenomenal description of the observation would mention only white streaks in a cloud-chamber.

What then is the distinction which quantum theorists intend to make when they introduce the word 'observable'? I want to try to show what it is and that it has no bearing whatever on the epistemological problem already mentioned, although it may have important consequences for the philosophical problem of causality, but this cannot be discussed here.

Some quotations from an article by Heisenberg¹ will introduce some epistemological concepts in the context of an examination of the development of quantum theory:

The single quantum jump of Bohr, Kramers and Slater is 'factual' in nature; it 'happens' in the same manner as an event in everyday life, or the deflection of a galvanometer (p. 13).

All the opponents of the Copenhagen interpretation do agree [that] . . . it would . . . be desirable to return to the reality concept of classical physics . . . that is, to the idea of an objective real world, whose smallest parts exist objectively in the same way as stones and trees, independently of whether or not we observe them (p. 17).

Whereas positivism is based on the sensual perceptions of the observer as the elements of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e. the actual, as the foundation of any physical interpretation (p. 22).

The 'actual' plays the same decisive part in quantum theory as it does in classical physics. . . . If we attempt to penetrate behind this reality into the details of atomic events, the contours of this 'objectively real' world dissolve—not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of a mathematics whose laws govern the possible and not the actual (p. 28).

¹ "The Development of the Interpretation of the Quantum Theory." In *Niels Bohr and the Development of Physics*, ed. Pauli, London, 1955, p. 12.

And from Reichenbach:¹

Using the word 'observable' in the strict epistemological sense, we must say that none of the quantum mechanical occurrences is observable; they are all inferred from macrocosmic data. . . . There is, however, a class of occurrences which are so easily inferable from macrocosmic data that they may be considered as observable in a wider sense. We mean all those occurrences which consist in coincidences, such as coincidences between electrons, or electrons and protons, etc. We shall call occurrences of this kind *phenomena*. The phenomena are connected with macrocosmic occurrences by rather short causal chains; we therefore say that they can be 'directly' verified by such devices as the Geiger counter, a photographic film, a Wilson cloud chamber, etc.

Several points emerge from these quotations:

(1) It is clear that the 'fact' words; 'observable', 'actual', 'real'; 'objective', are being used to refer to some events which are interpreted in the language of quantum mechanics and are therefore not reported in theory-free statements, and that the epistemological status of these events is regarded as equivalent to that of deflections of a galvanometer, stones, trees, etc.

(2) The theoretically interpreted events of which this is true are, generally, those which can be connected to phenomena (in the sense of this paper, not of Reichenbach's definition) by inferences using exclusively classical physics (a charged particle causes a scintillation on a screen whether in classical or quantum physics, and therefore the observation of a scintillation can be said to be an observation of the objective coincidence of a charged particle with the screen, whatever else may be said about this charged particle in any particular case).

(3) The distinction between objectivity and mental construction (actuality and potentiality in Heisenberg's language) is *not* the same as in positivism, where only sense perceptions are 'objective'.

(4) The 'details of atomic events' do not cease to be 'objective'; they merely cease to be describable *in the same way* as 'stones and trees, independently of whether or not we observe them'.

What emerges from this about the status of statements involving concepts of classical physics is quite consistent with what has been said

¹ *Philosophic Foundations of Quantum Mechanics*, Berkeley and Los Angeles, 1944, p. 20

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above on the relation between theoretical and phenomenal statements, and brings out the point that difficulties about 'unobservable entities' in the technical sense are peculiar to quantum physics. What these difficulties are, and how they differ from the traditional phenomenalist difficulty will now be considered.

Mach and Pearson regarded such entities as 'atom' and 'aether' as unobservable because they were not direct implications of sense-impressions; because they could not be seen, felt, tasted, smelled, or heard. But in ordinary language this is not sufficient reason for refusing to use the word 'observe' or language referring to the senses. We might speak of seeing aircraft when the only phenomena present are vapour-trails, and of speaking to X when we hear only a voice on the telephone. Perhaps we should not do this if we had never seen aircraft on the airfield or X sitting in our drawing-room. But this is only to say that mention of aircraft or of X implies that there will be other manifestations of them in given circumstances; that is they are not just *ad hoc* constructions out of vapour-trails and voices. And in classical physics there are many well-defined ways in which atoms and aether manifest themselves, all consistent with the assumption that they have the characteristics between observations which ordinary physical objects are assumed to have, namely that they behave according to the relevant laws of nature irrespective of whether they are observed or not. The fact that they manifest themselves in different ways consistently with these laws of nature means that they are not *ad hoc* constructions, and the fact that we never meet an atom face to face or go for a swim in the aether does not mean that we have no right to regard them as objective entities. It is just that they are not the sort of entity that one does meet face to face or swim in. The laws of nature which are relevant to them are not necessarily identical with those of physical objects, although, as with physical objects, these laws can be regarded as consistent descriptions of the atomic entities when these are unobserved.

Such is the epistemology which Heisenberg and Reichenbach, in contrast with Mach and Pearson, wish to employ when speaking of the entities of classical physics, and of modern physics in so far as these behave classically. The epistemological difficulty here is one which is common to classical and modern physics and turns on the propriety of using the word 'observe' when reporting the results of experiments in theory-loaded terms. We have already seen that experiments have to be so reported, but it may still be objected that the theoretical nature

of these reports should be safeguarded by avoiding the suggestion that atomic entities etc. are being directly observed.

I am inclined to think this objection ill-founded, and will try to show why by examining some remarks by Professor Körner.¹ After defining 'ostensive rules' as those which 'all or almost all human beings (after a certain stage in their development) accept' (p. 7), he speaks of passing from 'signs the use of which is governed by ostensive rules to other discursively used signs such as . . . "electron". . . the latter cannot in any straight-forward manner be regarded as applicable to anything' (p. 17). But he also thinks that 'many such scientific concepts . . . are only provisionally non-ostensive. . . . An atomic nucleus might conceivably some day be pointed-out' (p. 52), and 'It is in principle possible to observe atoms directly' (p. 60), although later he briefly suggests that talk of 'what is in itself unobservable' being indirectly observed through its effects, or inferred from what is observed, is unclear and would be better expressed differently (p. 193).

If only ostensive rules which are universally accepted are to count, then indeed atomic entities are non-ostensive, but it is not clear how in this case they could ever be pointed out as Körner suggests. An atomic nucleus is not the kind of thing one could unmistakably meet in the street, and if what is meant is that it could be pointed out in something like an electron micrograph, then it is still only the physicist who can interpret the marks there seen as an atomic nucleus, and this interpretation is no different in principle from the interpretation of cloud-chamber tracks. It might of course be the case that human beings universally learnt the physicist's language and shared his laboratory experiences, but if this is what is meant it seems that the distinction between ostensive and non-ostensive is mainly sociological and only logical in the sense of referring to different interpretive levels, as Körner himself explains in connection with ordinary physical objects (*ibid.*, p. 136 sqq.). It is admittedly easier to be *mistaken* about highly interpreted observation, but this does not show that it is not observation, for observation, as we have seen, is never incorrigible.

The distinction which is often implied between observing atomic particles 'directly' in micrographs, and observing them 'indirectly' by means of cloud-chambers or Geiger counters seems to be a misleading extension of the analogy with physical objects, and one which does not clarify the logical function of observation in relation to theories. The distinction is made plausible in the first place because in micro-

¹ *Conceptual Thinking*, Cambridge, 1955

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graphs, if we assume the analogy between atomic entities and ordinary particles, we can be said to be 'seeing' the atom in the same sense as we 'see' an object x in its photograph. The relation between x and its photograph expressed in terms of light or electron beams is the same as that postulated in the particle model between the atom and its photograph, even though in the case of the atom nothing corresponds to 'seeing' x in the straightforward sense. In the case of cloud-chamber tracks of electrons, however, we may be more reluctant to use the word 'see' because in the analogous case of aircraft making vapour trails, we might prefer to say that the aircraft was the *cause* of the trail, not that we saw the aircraft directly. But this is because we know what it is to see the aircraft directly. In the case of electrons, photons, and mesons we do not know what it is to see them directly in this sense, in fact such a 'direct seeing' is in principle impossible, at least in the case of photons, because they are the very media by which we see directly, and no mechanism of so seeing them can theoretically be conceived. Are we then forced to say that all observation of them is 'indirect', whereas observation of an atom in a micrograph is 'direct'? This does not seem to be necessary. Each entity is observed in the ways appropriate to it and not in ways which are in principle impossible, and the distinction suggested does not point to any important logical difference in the way the entities function in the theory.

This distinction must not be confused with another which is logically important: namely that concerning observations which are said to constitute the 'discovery' of a new particle. The neutrino was properly said to be 'discovered', when independent means were found of detecting the presence of a particle which had previously merely been postulated to account for lack of energy balance in certain nuclear reactions. Until these means were found the particle might have been merely an *ad hoc* construction invoked to save the conservation of energy, but to show that it was not that, it was not necessary to 'see it directly' in any kind of photograph, but simply to detect it in some way appropriate to the detection of sub-atomic particles, and the detection was in fact in terms of tracks. It is the independence of various observations which ensure the 'real' (non-*ad-hoc*) character of atomic entities, not any distinction between direct and indirect observation.

To return to the physicists. What they wish to contrast with the observable is not the *unobservable* but the *unobserved*. Their difficulty about the unobserved is not a general difficulty about theoretical

concepts as such, but a particular difficulty about the quantum theory. Reichenbach explains what it is by pointing out that in the usual cases of physical objects and the entities of classical physics it can be consistently assumed that the laws of nature relating to them are the same whether they are observed or not, and that if there is no external influence upon them other than the very small influence necessary for observation, their state remains the same. (The tree is always about in the quad.) If an unobserved system can be so described, Reichenbach calls it *normal*. It can however be shown to be *self-contradictory* to assume this in the case of the particles of quantum physics when they are unobserved, since the occurrence of an observable event in connection with them (coincidence with a photographic plate, etc.) changes their state in unpredictable ways. What is unobservable is not the particle as such, but its state when unobserved. This sounds like a tautology, but is not, because it may be said of a billiard-ball that its state is observable when unobserved in the sense that its position and momentum can be consistently inferred from their values when observed, and that another observation will confirm this inference. This is not the case with atomic particles. It is important to notice, however, that this conclusion follows only if the analogy between microscopic and macroscopic (mechanical) events is assumed, that is, if the particle-model for microscopic events is assumed. It is not *logically* impossible that a quite different theory of the microcosm might enable all relevant events and entities to be described in terms of what Reichenbach calls normal systems. But these events and entities could not be particulate in the classical sense.

I conclude that the use of the term 'observable' and its cognates by physicists in quantum-mechanical contexts has no special bearing on epistemological controversies. What is epistemologically interesting about quantum theory is that it provides a clear example of the inadequacy of the dictionary theory, and has to be described in terms of a hierarchy of interpretive levels, each introducing a language richer than that of the preceding level. It has been the object of this paper to examine the nature of such a description.

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THEORY-CONSTRUCTION AND THEORY-TESTING *

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I wish to defend 'the dictionary view' of scientific theories against Miss Hesse's attack and to argue that she has insufficiently considered her assertion that phenomenal statements do not 'constitute a basic empirical language which is independent of all theory'. I agree that the word 'dictionary' is likely to mislead in this context and that the 'theory-loading' view of phenomenal statements, much in vogue among Angry Young Philosophers, contains a grain of truth. But, on the whole, I am a reactionary in these matters.

So reactionary am I that I would be prepared to talk of observation statements, but I defer to Miss Hesse in this. However, I should like to introduce another term. Miss Hesse means by 'phenomenal statements' ('almost always') those which 'do not describe what happened on a particular occasion to a particular observer, but what always happens and will happen on sufficiently similar occasions to all normal observers' (p. 15). Since the testing of theories is done by individuals who can only be in one place at a given time, I wish also to refer to 'test statements'. These are whatever singular statements are used, in the last resort, to test a theory, are directly verifiable and, at their simplest, are those singular statements which can be deduced from Miss Hesse's general phenomenal statements together with singular existential statements. Depending on the context, test statements may contain technical terms of some scientific theory or they may be in the simplest common-sense language understandable by all normal people. In happier circumstances I should call these last, at least, 'observation statements'.

Miss Hesse is, it seems to me, playing the part of Prospero; with her permission I should like to cast myself for the part of Caliban. The root of our disagreement lies in our taking views of scientific theories appropriate to these characters. Prospero sees science as a body of

* Paper read in reply to Parts 1, 2, and 3 of Dr M. B. Hesse's article at the second annual conference of the Philosophy of Science Group, Nottingham, 21st September 1957.

theory to be tested and asks, primarily, 'Does my magic work'? Caliban sees, first, a collection of puzzling phenomena which he would like explained and asks 'How can this be accounted for?' Prospero knows 'how to bedim the noontide sun and call forth the mutinous winds': Caliban knows where to find fresh springs, pig-nuts, and marmosets. Prospero puts his faith in his theories and grudgingly admits phenomenal statements because of their superior certainty: Caliban takes phenomenal statements as basic because they are what he wants explained. This is a difference of emphasis which has far-reaching effects. If we regard a phenomenal statement as primarily something for testing theories we are likely to reach conclusions about it different from those we reach if we regard it as something which needs explaining. But phenomenal statements must be accepted, in the first place, not because of their superior certainty but because they are what the scientist sets out to explain.

That Miss Hesse is correctly cast as Prospero emerges throughout her paper. She is almost entirely concerned with the testing of theories and hardly mentions their explanatory function. Because she works always from the theory downwards to the phenomenal statements she sees phenomenal statements as always involving the terms of the theory.

I do not wish to argue that Miss Hesse's view is entirely unjustified. The scientist is neither Prospero nor Caliban; he is Prospero-Caliban. I wish merely to plead for Caliban. The scientist is a dreamer of dreams who finely spins theories but we must never forget that he is also a grubby, worried little man who stains his fingers and drops weights on his toes. If we ignore either of these things we get, at the best, a one-sided picture of science and this, I suggest, is what Miss Hesse has done. There are two sorts of operations in which the scientist engages; first, that of noting puzzling phenomena and searching for explanations of them and, second, that of looking for, or trying to produce, other phenomena which will support these explanations. I shall argue that the descriptions of phenomena involved in the first pursuit logically cannot, in one sense, be theory-loaded and that if those involved in the second pursuit are often, in practice, theory-loaded they are, and must be, capable of being unloaded.

Science begins when we begin to search systematically for explanations of phenomena. The original question is 'How does that happen?', which any man, call him A, may ask about something he sees. His companion, let him be B, may ask 'What are you talking

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about?' and then A has to describe what he sees. 'This stick is straight but it looks bent when partly immersed in water.' And B, if he is neither blind, nor illiterate, nor an idiot, will say 'Oh yes, I see, how odd'. This, incidentally, is the point at which all the fuss about basic propositions or protocol statements is relevant. It has nothing to do with whether a description of an experiment can be given without using scientific technical terms. It is concerned with whether or not we can be certain of agreeing about the phenomenon before ever an explanation is sought for or proposed. As I do not think this has the slightest relevance to our main theme, I shall not dwell on it.

But to return to A and B. So far they are Calibans, puzzled by what they have seen. Now they may begin to try various explanations, and the question they ask about each is 'Does this explain it?' *not* 'Does what we see support, or test, our theory?', since there is no point in trying to *support* the theory unless it can do the primary, explanatory job.

Now, it may be argued that in every example even the original description of the phenomenon is theory-loaded, in the sense that the terms 'straight' and 'bent' depend upon some primitive geometrical theory *or* that the peculiarity of the phenomenon is only understood if it is already accepted as a law that sticks that look straight normally go on looking straight. The first point has no substance since 'straight' and 'bent' can surely be regarded as names for certain appearances, unconnected with any theory. If the second point indicates theory-loading it is of a sort which is unobjectionable. All we need demand is that the original description of what is to be explained must not be loaded *with the theory which is intended to explain it*. This is my first main point.

Our description of a given phenomenon may be altered when we accept a theory as explaining it but the new description cannot function as a test statement for *that* theory. It may, of course, function as a test statement for a new and wider theory but, again, it must not be stated in the terms of that new theory. Depending on the theory in question a test statement must be a description which either is not theory-loaded or is loaded only with accepted theories which are not under test. Such theory-loading is not fatal to science as an explanatory system as, I submit, Miss Hesse's view is.

I say it is fatal on logical grounds. We must remember that to say that a statement confirms a theory *is* to say that it is explained by that theory. In general, if *e* is to explain *p* then *e* and *p* cannot be identical

nor can either contain the other except in the sense in which the premisses of a deductive argument contain the conclusion, otherwise we have not got an explanation. The explanation of something must be different from that something. Applying this to scientific theories we can say that technical terms invented to explain our description of phenomena must not be included in that description. That is, the technical terms of a theory which have been introduced *just for the purpose* of explaining *p* must not themselves appear in *p*, although other technical terms of already accepted theories *may* appear in *p*. If this condition is not fulfilled the theory will not explain *p* and *p* will not serve as a test of that theory.

Consider a trivial example adapted from Campbell.

p: 'When milk freezes it pushes the top off the bottle.'

e: 'That is because milk, unlike other liquids, expands over certain temperature changes.'

This is a primitive explanation but it would not explain *p*: 'When milk freezes it *expands* and pushes the top off.' An explanation of this would require a statement which did not depend upon expansion but *explained* this expansion. On these grounds I conclude that if Miss Hesse is correct in holding that any description of phenomena which is to test a theory must be loaded with that theory, then a scientific theory can never function as an explanation. I am not sure that she wishes to make this last assertion.

When Miss Hesse says that 'the claim that phenomenal statements are unambiguous cannot be maintained' she is, I think, confusing doubts about whether phenomenal statements can be certain with doubts about whether they depend upon any scientific theory. That is, she is irrelevantly bringing in arguments about protocol statements. I am not sure who, nowadays, would say that phenomenal statements are unambiguous but it is certainly not implied, as she says it is, by 'the assertion that the meaning of the statements is independent of any theories' (p. 15). There are other ways of being ambiguous than by being theory-loaded. But, more seriously, I am not sure that anyone has asserted 'that the meaning of the statements is independent of *any* theories' and no adherent of the dictionary view *need* make it. Campbell says 'it must be possible to determine, apart from all knowledge of *the* theory, whether certain propositions . . . are true or false.'¹ This means, I suggest, that the test statements for a given theory

¹ *Physics: The Elements*, Cambridge, 1920, p. 122. Quoted by Miss Hesse, p. 12 (my italics)

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must not be loaded with *that* theory, which is what I have been asserting. I do not think that the other four versions of the view make the mistake Miss Hesse accuses them of but, even if they do, it is not inherent in the view that they must.

*I am not convinced that Miss Hesse has been wise in grouping these five statements of the view together and discussing them as if they were one. I have time only to take as an example the versions of Campbell and Ramsey, and to insist that Ramsey did not follow Campbell as closely as she seems to think. Their 'dictionaries' serve quite different purposes. While Campbell might,¹ I think, accept with reservations that 'the meaning of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements', Ramsey gives *his* dictionary² the different function of connecting two parts of an abstract system neither of which, at this stage, contains phenomenal statements. His dictionary gives definitions of formulae of one part of the system in terms of formulae of the other part. Either part may then be *interpreted* in phenomenal terms but this is not done with the help of the dictionary. For Ramsey, defining and interpreting are different processes. He perhaps obscures this to some extent by giving, for pedagogic reasons, an example of a possible interpretation of the system in the course of constructing it. But he is concerned, like Braithwaite, to build up a *calculus* which shall show the logical structure of a scientific theory. When this calculus is interpreted in terms of experiences it will be shown to apply to existing scientific theories. But the dictionary belongs to the calculus and is not a tool for interpretation.

Braithwaite and Woodger are, I think, closer to Ramsey, Ayer closer to Campbell in this, though I doubt if any of these authors would accept either Campbell's odd view of meaning or his anti-logical assertions of Chapter II. I shall not, however, press my demand for separate treatment of these versions since Miss Hesse's main criticism of the dictionary view, as she conceives it, is, I wish to argue, without foundation and does not render any of these versions untenable.

The two alleged features of this view upon which she bases her criticism are:

'that the meaning and criteria of truth of phenomenal statements is independent of that of theoretical statements' and 'that the meaning

¹ Op. cit., p. 122-123

² *The Foundations of Mathematics*, London, 1931, pp. 214 sqq.

of theoretical statements is dependent upon a dictionary which translates some of them into phenomenal statements' (p. 14).

Leaving aside the difficulties about the uses of the dictionary, the second alleged feature needs much refining to make it fit, for it is possible to say that theoretical statements have *some* meaning (mathematical, analogical, etc.) apart from those phenomenal statements they go towards explaining. But this need not delay us since Miss Hesse deals mainly with the first of these features which is a claim of all five versions of the view.

The criticism which Miss Hesse regards as fatal does not touch this view or any of its implications. She argues that 'if phenomenal statements are to be tests of a theory they cannot be independent of the theory' (p. 16). If phenomenal statements are to have scientific significance 'there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped' (p. 16). 'Physics can be reduced to phenomenal statements if these connections of meaning are ignored, but such reduction does not give a full account of the observational basis of physics, for it cannot explain why just these statements are chosen and not others' (p. 16).

Now, in the first place, if Miss Hesse is arguing against reductionist accounts of scientific theories I am with her, and so, I should have thought, were Campbell, Ramsey, Braithwaite, Ayer and Woodger. This is surely an important feature of the dictionary view. The assertion that phenomenal statements are deducible from sets of theoretical statements and existential statements does not imply that these theoretical statements are translatable without remainder into phenomenal statements. Campbell claims that theoretical statements are significant apart from the dictionary and only some of them translatable.¹ Ramsey was precisely concerned to dispute the reductionism implicit in the logical constructions view. Ayer rules out reductionism in his explanation of 'directly verifiable'.² Professors Braithwaite and Woodger can no doubt speak for themselves on this.³

But, in the second place, and more importantly, surely none of these authors denies that there are connections of meaning between phenomenal statements at a higher than common-sense level, nor is such a denial implicit in their accounts. Such connections are precisely what

¹ Op. cit., p. 122

² *Language, Truth and Logic*, London, 1950, p. 13

³ But see Braithwaite: *Scientific Explanation*, Cambridge 1955, pp. 50-53

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scientific investigation tries to establish and this is one of the features to which the dictionary view tries to do justice. In his discussion of the Kinetic Theory, Campbell brings out the way in which the Theory shows that connections between perfectly elastic particles underlie, for instance, connections between the pressure, volume, and temperature of a gas. This is allowed for in his account of theories. The higher than common-sense connections are to be found among his 'hypotheses'. These must be significant *apart from the dictionary* but, apart from the dictionary, appear to be arbitrary assumptions. By relating *some* of the hypothetical propositions to *some* phenomenal statements the dictionary removes this arbitrariness. The connections between the hypothetical propositions, some of which imply and some of which do not imply phenomenal statements but all of which are significant, *are* the higher than common-sense connections between those phenomenal statements. Much the same sort of thing can be said about the other versions.

Of course Miss Hesse is right when she says that if phenomenal statements are to be tests of a theory they cannot be independent of the theory, but whoever thought they could? What is asserted is something quite different, namely, that if a phenomenal statement is to be a test of a theory it must be understandable without reference to *that* theory. Showing that a statement which we understand is derivable from other statements shows us more about its connections with these others but it does not lead us to understand anything more or different by *that* statement. The meaning of 'Socrates is mortal' is not changed when we discover that it can be deduced from 'All men are mortal and Socrates is a man'. There must, of course, be connections of meaning between phenomenal statements and the theory that explains them otherwise the theory would not explain them, but this does not imply that the meaning of a statement is altered when a theory is found to explain it or that it cannot be understood without knowledge of that theory. That this is so is abundantly evident in the history of science, for many connections on the phenomenal level have been known before any theory was invented to explain them. Boyle understood by the statement that $p v = \text{constant}$ just what Clausius and Maxwell understood by it even though Clausius and Maxwell knew more about the possible *explanations* of it.

Miss Hesse is in her most Prospero-like mood when she enlarges on her view that 'a phenomenal statement can never test a theory alone, but only a theory together with its dictionary interpretation into this

particular phenomenal statement' (p. 16). This, and much else, suggests that scientists sit in their studies spinning theories out of thin air and then set about looking for phenomenal statements to test them. In consequence, she is worried about how we know that a given interpretation of a theory into phenomenal statements is relevant to the theory. But surely the way in which we start from puzzlement about phenomena ensures that our theory will not be wildly irrelevant. Consider Miss Hesse's example of Snell's Law. We know how to interpret $\sin \alpha / \sin \beta = \mu$ because we arrive at this constant relation by examining the phenomenon, drawing diagrams and measuring what appear to be relevant angles. In a sense, we *begin* with the interpretation and work upwards to the formalism. We choose *this* formalism because we have arrived at it through the phenomenon. So it is, I suggest, with most theories, however much more complicated they are than this. (See Braithwaite's zip-fastener analogy.) Miss Hesse puts the cart before the horse. The possible interpretations of α and β are not, at this stage, 'already circumscribed by the theory' or, at least, this is a misleading way of putting it. The theory is circumscribed by the context of the observations which lead to it.

When we come to test the theory by other phenomena, such as the burning of paper by a lens, we, of course, test our interpretation in a new field and here the interpretation *is* circumscribed by the theory. But what we test is whether the phenomenal description follows from an account of the bending of light rays. It is the relation between the description in terms of the theory and the ordinary phenomenal description, using none of the terms of the theory, which is important. Approximate judgments of relevance which guide any variation in interpretation for the new phenomenon are, as Miss Hesse says, common-sense judgments: the testing of the theory is itself the determining of the precise relevance.

Miss Hesse's further example concerning scintillations and the neutrino (p. 18) can be similarly dealt with, although it appears more favourable to her view simply because it concerns a 'higher level' in the theory. Relative to the theory concerning the presence of an undetected particle, the description of the scintillations together with the account of decay processes in the reactor is a basic or test statement. That is, the observations plus the established theory are used to test an *addition* to that theory, but what is directly observed (scintillations) could be described without reference to any part of the theory, and *this* description would be a test statement for the *whole* of the theory.

THEORY-CONSTRUCTION AND THEORY-TESTING

In her remarks about the teaching of the neutrino theory, Miss Hesse has, I think, failed to distinguish between understanding a test statement, which does not require knowledge of the theory, and understanding that it is a test statement for that theory, which does require such knowledge.

I find her conclusions about the Wilson cloud experiment exceedingly puzzling. The phenomenal description of apparatus, lines drawn and angles measured, she says (p. 20), 'clearly . . . means nothing and could test nothing, because it does not state its relevance to anything'. But in the context in which it is likely to be thought to test a theory, there would already be reason for thinking it relevant to the theory. The test would determine its precise relevance by showing it possible (or impossible) to deduce from the higher level statements that if the observer, under certain conditions specified in phenomenal statements, drew certain lines and measured certain angles he should get the observed values. The theory has, after all, been constructed from *other* basic phenomenal statements which have led to its mention of photo-electrons and so on. The context of the new observations suggests its application to this phenomenon and what we test is the relevance of this suggestion. But the fact that just above the new phenomenal statements in our deduction we have non-phenomenal statements does not make the phenomenal statements any the less phenomenal. We can, as it were, 'shed' the theoretical terms as our deduction proceeds until we finally reach statements about the results we should get if we draw lines and measure angles. There is no need for a phenomenal statement to proclaim its own relevance to the theory. The theory proclaims its relevance to the phenomenal statement if it fits.

Campbell's primitive theory about the resistance and temperature of metals brings this out.

$$(c^2 + d^2)a \left| \frac{cd}{b} = 2ab = \text{const. is deduced from his hypothesis.} \right.$$

By means of the dictionary it is interpreted as 'the ratio of the resistance of a piece of pure metal to the absolute temperature is constant'. Given that the meaning of all these terms are accepted by us we need no further translation but with the help of these accepted meanings we can translate it further, for the layman, into purely phenomenal terms, i.e. terms which do not require knowledge of the scientific theories of electricity, temperature, or chemical purity. This is all deductive, and

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the fact that our deduction takes us through statements which cannot be understood without scientific knowledge does not imply that the lowest level statements cannot be so understood. Moreover, the fact that Campbell's theoretical statement can be translated into phenomenal statements does not imply that all his hypothetical statements can be so translated, for some of them may function purely as steps in the deduction near the top of the chain and not 'come out' at the bottom. All this is brought out also by the systems of Ramsey and Braithwaite.

There are more ways than one of giving a commentary on what scientists do. Miss Hesse has, I think, a tendency to regard her five authors as doing some kind of psychologico-historical study of science, *to see them thinking as scientists think*, and this has led her to misconceive their results. They are claiming *not* to show how scientists work but to present a 'rational reconstruction' of their finished theories. They are not claiming that scientists 'set up a theoretical deductive scheme' (p. 17) and then search for possible interpretations of it but rather that such theoretical deductive schemes can serve as models which exhibit the logical relations between the statements from which they start and those with which they conclude. Campbell and his descendents emulate neither Prospero nor Caliban, but Shakespeare.

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NOTE AND COMMENT

On Multi-dimensional Time

MULTI-DIMENSIONAL time variables have been discussed in this *Journal*¹ in connection with highly controvertible psychological speculations. Here I just wish to point out that two-dimensional time and, more particularly, complex time need not be introduced *ex hypothesi* in physics, being already included in theories of spinning particles.

In fact, it has been shown² that six new constants of the motion of the free electron are obtained by performing a certain transformation involving the new co-ordinates

$$X^\mu = x^\mu + (\Lambda/2)i\gamma^\mu, \Lambda = h/m_0c, \mu = 0, 1, 2, 3,$$

where $i = \sqrt{-1}$, 'h' designates Planck's constant divided by 2π , 'c' represents the velocity of light in vacuum, ' m_0 ' stands for the electron rest mass, and ' γ^μ ' denote Dirac's 4×4 matrices. The eigenvalues of the space part, $\mathbf{X}$, of this operator are real and may be interpreted as the centre of mass co-ordinates of the electron, the mass of which (but not the charge) can be pictured as spread over a sphere of average radius $\sigma = \Lambda/2$. (The factor $1/2$ drops out in the case of spin 1 particles.) On the other hand, the eigenvalues of the time component of that operator are complex:

$$\text{eig } X^0 = ct \pm i(\Lambda/2)$$

The constant time $\tau = \Lambda/2c$, the value of which is of the order of 10^{-21} sec. in the case of the electron, may be interpreted as the spinning period of the electron charge.

As can be seen, there is no mystery in the above complex time operator X^0 ; it just represents a pair of real times, to wit, the extrinsic time variable t , and the constant intrinsic time τ , related to the internal motion of the particle. Complex quantities entering physical theories are—provided they do not play a barely intermediary syntactical rôle—just pairs of real quantities having each a physical correlate. Nothing unreal and nothing psychological is implied by their use.

MARIO BUNGE

¹ H. A. C. Dobbs, 'The Time of Psychology and of Physics', this *Journal*, 1953, 4, 162. C. T. K. Chari, 'A Note on Multi-Dimensional Time', this *Journal*, 1957, 8, 155

² M. Bunge, 'A Picture of the Electron', *Nuovo Cimento*, 1955, ser. X, vol. 1, p. 977. Preliminary results on similar work concerning spin 1 particles were reported by the author in 'Sobre la imagen física de las partículas de spin entero', *Rev Unión Matemática, Arg. y Asoc. Fís Arg.* 1957, 18, 74.

DISCUSSION

ON THE SPACE AND TIME OF IMAGES

THE purpose of this note is twofold: (1) to reply to certain points raised by C. T. K. Chari in a recent note in this *Journal*¹ and (2) to make certain modifications to my theory suggested by Professor Broad.

1. Chari states that we have no criteria for judging the geometry of hallucinatory spaces and that we have no measurements or conventions to determine the 'metrisability' and dimensionality of the space of mescal visions. In which case we should not talk about an image space as though it could be conjoined in some way with physical space, for which we do have such conventions and means of measurement. To approach this let us distinguish a hierarchy of questions: (a) 'Are visual images (I include here visual hallucinations, mescalin-induced images, hypnagogic and eidetic images) in some sort of space?' This may be put 'Do visual images have (b) topological and/or (c) metrical properties?' I would argue that images *do* have the topological properties that they appear to have on simple inspection. For instance an image has a spatial *boundary* that uniquely divides the field in which it is embedded into one *inside* and one *outside*. Professor Price² has emphasised that one image can bear various spatial relations such as 'to the left of', 'above', 'below', etc., to one or more other images and Nicod³ has minutely specified the spatial relations of sense-data. Two hallucinated squares cannot intersect more than twice any more than can two squares drawn on paper, whereas an hallucinated square and rectangle can intersect up to four times. We should say that images and hallucinations *satisfy* some simple axioms and theorems of topology. It would be interesting to observe how many of the more complex theorems are also satisfied by images.

The next question is 'Is such a space metric?' Again the answer seems to be 'yes', for the metric of physical space is *based* ultimately on such perceptual observations as 'a is longer than b', or that a certain mark on a scale is opposite an end of something, or that the tip of a pointer is opposite a mark on a dial. Physics reflects the complex numerical relations obtainable in this way. Now, if we examine any field of hallucinatory images, we will also be able to discover that the same relations hold for this field, i.e. that one image is much smaller and shorter than another and that one mark is opposite another. The numerical relations between the lengths and relations of such images may only be confused, but this only means that we are not able to build a physics out of our observations; it does not mean that the entities observed do not possess a geometrical order themselves. We cannot take any

¹ C. T. K. Chari, 'On the "Space" and "Time" of Hallucinations', this *Journal*, 1958, 8, 302-306

² H. H. Price, 'Survival and the idea of "another world"': *Proceedings of the Society for Psychical Research*, 1953, 50, 1-25

³ Jean Nicod, *Foundations of Geometry and Induction*, London, 1930

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particular sense-datum or image as a unit of image length (as we can take a convenient stick) because the definition of any *particular* image or sense-datum is valid only for the instant at which it is made. Sense-data are shifting parts of a more permanent whole—the visual field—as images are part of what we can call the image field—and the only metrical judgment that can enable us to compare sense-data not compresent is the relation to the whole visual field, i.e. the amount of the total field that they cover. Chari objects that we cannot metrise image space because of the unstable character of sense-data. But this objection merely shows that we cannot use one sense-datum as a permanent rule to measure other sense-data, and that Chari has not analysed the concept of metrisability at a sufficiently fundamental level. We can accept here Dingle's definition¹ of measurement as any precisely specified operation that yields a number. Clearly such operations can be carried out on images and hallucinatory sense-data as I have described above and in more detail elsewhere.²

So we can establish, on the basis of a mere inspection of images, that they do have some topological and geometrical properties, i.e. that image space has *some* kind of geometry. But it may not be legitimate to ask if its geometry is Euclidean or non-Euclidean. These concepts may express levels of relationships at a level higher than we can go with the primitive contiguities and congruences of images. The sum of the three angles of an imaged triangle add up to something like 180° but we cannot be any more specific than that. We can be certain, however, that at times we can observe sensory images that have three angles, others with four, five, none, etc. We can also say that image space is itself three-dimensional, for we need three and only three co-ordinates to specify the position of any hallucinatory sense-datum in an hallucinatory sense-field. In any case for the purpose of relating image space and physical space it is only necessary to establish that image space is a topological and metric space of some kind. The details of the spatial relations between images need not affect the gross specifications of the relations between images and objects.

If we then agree, after a close inspection of, say, a clear little hypnagogic image, that it does have both topological and metrical properties—that Klüver's spiral form constant is a spiral—we can then ask our next questions, 'Does this image bear (*d*) topological and/or (*e*) metrical relations to physical objects?' Professor Price³ has recently put forward an affirmative answer to question *d*. He postulated that images, although in a space of their own, bear no spatial (metrical) relations to objects but only causal relations. The topological relation of 'external to' is implied in this hypothesis. Lastly a 'yes' answer to question *e* may be amplified by noting the *possible* geometrical relations between one 3-spaced and another and by asking how many image spaces there may be, i.e. do we all share one or do we each need one to ourselves? Previously I had assumed that one needed a $(3m + 3)$ —space to accommodate *m* human individuals and their individual image spaces but this was an error as appears below.

2. Professor Broad, who was the first to suggest⁴ that the space of sense (and images) and physical space were some kind of section of a space-like whole of more than three dimensions, has pointed out that to accommodate physical space and *m* image spaces we

¹ H. Dingle, 'A Theory of Measurement', this *Journal*, 1950, I, 5-26

² J. R. Smythies, *Analysis of Perception*, London, 1956

³ H. H. Price, loc. cit.

⁴ C. D. Broad, *Scientific Thought*, London, 1923

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PROBABILITY AND INDUCTION

It is an undeniable fact that scientists agree fairly well in what they do in the laboratory but disagree very much in explaining what they do. Experimental procedure is hardly ever in doubt, but scientific method is. Scientists might use vaguely similar terms for characterising their methods, though what they mean by them are obviously very different things. Thus they say that their methods are *inductive* and that these establish the results of their research with a certain *probability*. For most scientists are conservative and follow the philosophical tradition in which they were brought up when they speak about their work, however revolutionary they are in the work they are doing. They say that they 'infer from the facts' and that, since facts are stubbornly independent of human beings, the inference confers only a certain degree of probability on the conclusion. What is meant by 'induction', 'inference', and 'probability' in this context is not at all clear. What is most irritating is that they are all good practical statisticians and use the mathematical calculus of probability with skill and exactitude. More often than not, they even manage to reach the same solution to a particular problem. But if one asks for an explanation of the method by which the solution was obtained, hardly one statistician ever agrees with another. On the contrary, statisticians are bitterly critical of each other and are prone to convict their colleagues of the basest fallacies. It is not only the layman who thinks that 'with statistics you can prove anything'.

The main reason is that, however technical and formal probability theory has become, the basic conceptions are beclouded by ancient metaphysics. Probability and statistics are supposed to be the tools of a so-called inductive inference; and 'Inductive inference is the only process . . . by which essentially new knowledge comes into the world' as Professor Fisher has remarked in one of his earlier books. With this (or some similar) statement we are led right back to about 400 B.C. when the problem 'What is Knowledge?' was first posed in a systematic manner. Its solution determines what we take as scientific inference or 'induction'. This is the ancient game of epistemology: the pieces we choose—that is, the key concepts—determine the rules of the game and vice versa.

The three books¹ under review are examples of this traditional game.

¹ R. A. Fisher, *Statistical Methods and Scientific Inference*, Oliver and Boyd, Edinburgh and London, 1956, pp. xiii + 175. 16s.

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Though they differ widely in their 'pieces' and in the conclusions reached, they agree in the attitude of *inductivism*. Let me add immediately that all three books are of the highest possible calibre and contain a great deal more besides inductivism: but most of it is severely technical, and I must confine myself here to some main topic of interest to the philosopher of science.

Professor von Wright, for example, discusses the philosophical history of the problem of induction and, in particular, the views of Hume and Kant. More than half of Professor Jeffreys's book is concerned with the application of scientific method to diverse fields ranging from Newtonian dynamics to quantum mechanics. Professor Fisher presents various forms of 'quantitative' inference in mathematical detail.

Two of the books, those of Jeffreys and von Wright, are new editions of well-known treatises which have been, however, partly re-written; while Fisher's book, though new, is a re-statement of ideas known to us from previous publications of the author. This is an additional reason for restricting the review to the main topic of probability and induction.

There are three main schools of thought regarding probability. One school identifies probability with relative frequency: thus, probability becomes a physical property of actual events, e.g. the probability of throwing a '6' with a die is characteristic of that die (and of the manner of throwing). The second school attributes probability to hypotheses, that is, to propositions about events. This view is exemplified by a statement such as this: 'It is probable that it will rain tomorrow.' Naturally, the probability of a hypothesis is usually *based* upon a relative frequency, e.g. the frequency of rain observed for the given date in the past fifty years in London; but it is not the same thing as that relative frequency. Probability, according to this school, is a logical and mathematical concept; it is often explained in terms of the logical range of sentences, that is, of the hypothesis and of the evidence.

The frequentists claim on that occasion that their kind of probability is 'objective' and that the logical concept is 'subjective' since it may be taken as referring to the credibility of a hypothesis. The defenders of the logical range theory point out that the frequency interpretation automatically precludes speaking about the probability of single events and that this is needed both in science and in everyday life. It is quite true, in my opinion, that relative frequency is merely a descriptive term summarising past events known to have occurred. If probability is to refer to something unknown—and this is, surely, what we want the concept to mean, at least, in part—then it must be more than a relative frequency.

H. Jeffreys, *Scientific Inference*, 2nd edition, Cambridge University Press, 1957, pp. viii + 236. 25s.

G. H. von Wright, *The Logical Problem of Induction*, 2nd enlarged edition, Blackwell, 1957, pp. xii + 249. 25s.

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These remarks must suffice to characterise the views of the two contending, theoretical schools. The third school is that of the practical statistician who rejects all discussion about the meaning of 'probability' as so much philosophy. He claims to take probability as an undefined notion and in this way to avoid the difficulties. In this he is, I believe, mistaken. It is true that we need not define 'probability' explicitly: definitions are not important in science. But we must give an axiom system and rules for the concept; and this constitutes an implicit meaning. Thus we are still confronted with the same task of finding out what the special interpretation is that we wish to give to the concept of probability.

The three works under review represent, *cum grano salis*, these main schools of thought. I shall start the discussion with von Wright's book since, of the three authors, he is most directly concerned with the philosophical issues involved. We find on p. 1 the definition of induction is given 'as reasoning from known to the unknown'; and it is the logical problem, that is, the relation of evidence to the induced proposition rather than the psychological problem of the discovery of 'invariances and laws' that the author wants to investigate. This brings in at once a question about the justification of inductive arguments, in the sense that such inferences must be justified 'before their actual experimental verification'. In other words, the attitude taken by von Wright is that of the empiricists and very close to von Mises and Reichenbach. Laws are considered to be universal statements but, all the same, factual. Thus it is natural to interpret 'probability' in terms of the proportion of elements in a class having a certain property, that is, as relative frequency. If inductive method aims at 'connecting two characteristics . . . by universal implication . . .', this move in the epistemological game is forced. Mathematically, the most important theorem of the formal calculus is that of Bernoulli, for without it frequency probability cannot be applied in practice. Professor von Wright formulates it in two parts, in a somewhat novel fashion; but even so he cannot avoid circularity, e.g. 'If the Bernoullian probability of the event is p , then the probability that the event's relative frequency on n occasions will deviate from p by less than an amount . . .' This difficulty is acknowledged by the author when he says 'that in the theorem mentioned *two* probabilities are involved'. A hierarchy of probabilities is needed in order to provide 'a bridge from the realm of probabilities into the realm of empirical frequencies . . .'. Clearly, this indicates the logical collapse of the frequency interpretation: for the concept of probability must be presupposed so that probability may be identified with the limit of relative frequency. The game of epistemology as played by the empiricist *v.* Nature ends in the defeat of the philosopher.

Let me add that this very brief summary does not do justice to von Wright's book; his arguments are often more subtle and acute than

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those of his fellow empiricists and range over a wide field. I take as an example the last chapter which is about induction as a self-correcting operation. This feature of inductive reasoning makes it more rational and, thus, superior to other kinds of argument about the unknown. In one sense, this is quite true. It is rational to correct one's mistakes, that is, to learn by trial and error : any method acceptable to science must satisfy this condition. But there is no single, privileged, method superior to any other, i.e. induction, by which this is *automatically* achieved. Such an assertion is of course no more than a weak form of the old Rule of Induction prescribing, *a priori*, the uniformity or regularity of nature without which we are not able, allegedly, to discover specific causal sequences. It has been called 'the faith of the scientist' or, in a more stream-lined formulation, it is the slogan that induction (like honesty, I suppose) is the best policy. Professor von Wright comes very close to saying that it is a tautology, namely, that 'The superiority of induction . . . is concealed in the *meaning* of the policy's goodness'. I would agree with this, for it implies that inductivism—the idea that there is a logical method by means of which new knowledge can be obtained—is, at best, an empty verbalism.

Hume's sceptical attitude to induction is thus supposed to have been overcome. 'To Hume the failure to justify induction seemed the discovery of a serious limitation in man's intellectual faculties.' According to von Wright, this failure says nothing about the world or the human mind : it is merely a consequence of our use of language, and this 'can truly be said to constitute the "solution" of the problem put to philosophy by Hume'. Thus the 'critical' task of inductive philosophy is separated from the 'constructive' task, the one dealing with the meaning of words, the other with the application of logic and mathematics to propositions about the world. This conclusion demonstrates, in my view at least, that induction, in the classical sense of Bacon and Mill, has to be abandoned : there is no logical method by which we can discover whatever is unknown, even though we start from what is known.

Of course, I do not want to suggest here 'that reasoning, properly speaking, cannot be applied to empirical data to lead to inferences valid in the real world'. This suggestion is 'horrifying' to Professor Fisher. I, too, should certainly want to reject it. But the question is : What is the nature of this reasoning ? 'The mathematical attitude to induction' is a phrase that is not sufficiently self-explanatory. We hear that both 'subjective ignorance' and 'objective knowledge' play a part in the probability statement. The knowledge 'refers to a well-defined aggregate, or population of possibilities within which the limiting frequency ratio must be exactly known', while the ignorance 'is specified by our inability to discriminate any of the different sub-aggregates having different limiting frequency ratios . . . '.

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The 'intuitionist' attitude of the practical statistician makes it immaterial that we can derive the laws of statistics from the set of axioms, as theorems; they can be accepted only if they are confirmed by experiment. Then we need special criteria to judge whether, and how far, the experiment is relevant to the hypothesis. Think only of the example of throws with a die. If we want to use them as evidence for the truth of statistics, then we must know beforehand that the die is unbiased. If we find that, in a large number of tosses, one side shows up with a frequency less than $1/6$, then we can take this either as falsifying the laws of statistics or as evidence that the die is biased. Which we do depends on criteria *external* to the statistics in question; we need extra knowledge if we are to decide whether or not to accept a statistical result as relevant. This is the reason why practical statisticians were forced to introduce tests of significance. It is a very technical matter, however; and I must therefore refrain from discussing Professor Fisher's very original ideas about such tests.

I have said enough already to show that Professor Jeffreys's views are almost diametrically opposed to those of Fisher. The first thing Jeffreys does is to provide us with an axiom set for the calculus of probability. And he goes on to say that the 'relation between a set of data and a conclusion is called *probability*, and the subject is essentially what is now called a many-valued logic'. Thus, his ideas are close to those of Carnap and the logical range theory, as he himself remarks. Frequency probability, pure and simple, has almost no application according to Professor Jeffreys; and he is severely critical about the logic its proponents have employed. I would agree with him here. However, I find it difficult to accept his view that all laws are probability statements. This is a view which fits in much more with the empiricist tendency of the frequentist. Of course, Jeffreys has a somewhat novel interpretation of what a law is. 'Most of them contain what are called adjustable parameters. For instance, "a mustard flower has six stamens" contains the number 6 . . .'. Similarly, he cites the law of gravitation as an example since the value of the gravitational constant 'has to be inferred from the observations'. The more usual view is, after all, that not all laws, say, of physics, are statistical, e.g. those of mechanics. And the numerical value of any constant occurring in a law is accidental: the constant must even be symbolised by a letter in the proper formulation of the law.

Professor Jeffreys's strictures of the hypothetico-deductive method arise from this interpretation, for he finds the method acceptable but incomplete. The method, he says, 'consists in stating laws for consideration, comparing with observation, and rejecting them if they disagree too badly . . .'. And he continues, 'Since errors of observation exist, any criterion for rejection must be quantitative; and there is no such criterion without at least a partial theory of probability But the statements of the

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hypothetico-deductive method mostly avoid speaking of the probability of a hypothesis and thus fail to deal with the problem of prediction'.

The difficulty comes here, I think, from the failure to distinguish between the law as a statement (or theorem) within a theory and its application in a specific instance. Professor Jeffreys cites with approval a remark (which, he believes, may be due to Fisher) that 'the standard error is just as essential a part of the law as the true value'. Every scientist would admit that, in one sense, this is so; but in another, it is not. Surely, we fondly hope that, with improved techniques, we obtain better data and so can show that our laws fit better than before. Indeed, I would heartily agree that the errors of observation determine the probability of a hypothesis (not of a law, really); but the errors are not a part of what the hypothesis is about.

Professor Jeffreys's views, I fear, are here coloured by a trace of inductivism. If probability is a logical and mathematical concept, then it cannot be *eo ipso* predictive. The world is not logical or mathematical, though our model of it is. It is the scientist who, by proposing a suitable model and by formulating a specific hypothesis on its basis, predicts the future or, in general, whatever is unknown. We see the future as a gamble and so the model we choose is that of the card pack, of the bag and balls, of dice throws, etc. Probability is the mathematical concept that is appropriate for such models.

For a similar reason Professor Jeffreys scornfully rejects the possibility that laws may be disguised definitions, that is, analytic statements (within a theory). 'Definitions of what? If there are n parameters in a law, n sets of values of the variables suffice to estimate them. The information that the law fits for a much larger set of values is ignored.' Now, it is true that many different types of statement are called 'law' in science; sometimes, we speak of an 'empirical law', which happens when we have no proper theoretical basis for it. Such a 'law' may be more like a factual hypothesis (within the usual context of its application). But how about Newton's first law? Where are the parameters in it, what is its probability, and who has ever found an exception to it?

The first edition of Professor Jeffreys's book has served several generations of scientists as a provocative and original introduction to methodology; the second edition will certainly fulfil the same purpose, enlarged as it is and including a new chapter on quantum mechanics. I cannot do justice to the wealth of material offered; and this applies equally to the books of Fisher and von Wright. In conclusion let me only say this.

Probability theory and statistics are still beset by many difficulties, and the main reason is that they are inextricably tied up with the philosophy (or metaphysics) of induction. Thus we find here the argument in terms of a dichotomy that has always been so typical of classical philosophy, e.g. objective *v.* subjective probability, events *v.* hypotheses, frequency *v.* range,

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justification *v.* application, description *v.* prediction, etc. Certainly, one thing that is true is that the meaning of one term of a dichotomy mutually depends on that of the other; thus we cannot resolve a dichotomy by simply cutting it into two pieces as is done by the opposing schools of philosophical thought.

The concept of probability has come to us from two sources—the general notion current in ordinary life and known to Aristotle as *eikasia* and the specific notion arising from the games of chance of the seventeenth century. The latter lends itself easily to mathematical treatment, since we have here a very restricted context, simple events, and well-defined rules. The situation to which probability calculus is to be applied is clearly specified, and so we obtain a certain model. It makes prediction almost automatic, since the games—like dicing or the drawing of balls from a bag—are played according to a rule. This brings about the unholy union of philosophical induction with mathematical probability. The union seems to realise one of the oldest dreams of mankind, that is, that mathematics can foretell the future and gain new knowledge for us.

Yet whatever probability may be, it is also a mathematical concept, no different in logical status from any other such concept, say, that of complex number: and no mathematical symbol is, by its very nature, predictive. Probability does no more 'change ignorance into knowledge' than $(a + bi)$ can. Why should a mathematical expression, e.g. the binomial theorem, automatically predict the future when it occurs in statistics, but not do so when used in arithmetic? No—we, the scientists, do the predicting; it is the model we choose; and very often we believe that the future is adequately pictured as the drawing of a ball from a bag, etc. To ascribe miraculous powers to probability is as wrong as to reject its use in scientific method (as some have done). If we can steer this middle course, we shall also manage to free probability theory from the spurious dichotomies that obscure it.

ERNEST H. HUTTEN

THE TELEVISION THEORY OF PERCEPTION

It has been suggested that vision involves a scanning mechanism which works on the same lines as the scanning mechanism used in the transmission and reception of television. Dr Smythies¹ uses this suggestion as the basis

¹ J. R. Smythies, *Analysis of Perception*, Routledge and Kegan Paul, International Library of Psychology, 1956. Pp. xiii + 140. 21s. Smythies gives a brief account of the evidence, pp. 69-71. A fuller account with references is given by W. Grey

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for a new scientific version of the old philosophical representative theory of perception, a version which, he says, can be tested by experiment.

1 *Smythies's Sense Data*

When someone looks at a light which is flashing at the rate of six to thirty flashes per second, complicated patterns appear in his visual field. When a television studio is illuminated by a light flashing at this frequency, similar patterns appear on the screen of the receiving set. Now it is known that the patterns which appear on the television screen are produced by the scanning mechanism, and it is concluded that the patterns appearing in the visual field are produced by something similar.¹ Smythies is not concerned with this scanning mechanism as such but rather with the similarities between visual fields and television screens themselves. He concludes from the above evidence that the visual field is a private screen of sense data which represents the physical objects of the outer world in much the same way as the television screen represents the physical objects which are being televised. The representatives of physical objects which appear to us on our private television screens are (Smythian) sense data. Smythies sets out to give a scientifically useful definition of the term 'sense datum', one which is free from the incurable vagueness which has pervaded its philosophical use. He does so by starting with an after-image in the visual field. Whatever has spatial relations with this after-image, whatever may be said to be in the same field, counts as a sense-datum. The definition is easy to understand and leaves us in no doubt as to what is to count as a sense-datum. Everyone knows how to obtain an after-image by staring at an electric light. Everyone can put the contents of his visual field into spatial relations with such an after-image. Smythies specifies more precisely in a series of mathematical definitions (pp. 6-7) given in terms of Jordan curves what 'having spatial relations' with an after-image is. If I have a patch of after-image in my visual field the boundary of that patch will naturally divide the field into two areas, one inside and one outside the patch. To say that the outline of the patch 'can be observed to describe a Jordan curve' is a pompous way of saying that it is such a patch. The definitions given of the relations between the patch and the rest of the field reiterate the same information. Their mathematical expression in definitions (I.1-4, pp. 6-7) is a feat of elementary translation from simple into technical language, but as these definitions are not used by Smythies in the development of his theory, we are left with the suspicion that their inclusion is an attempt to cajole us with technicality for its own sake.

Walter in *Perspectives in Neuropsychiatry*, edited D. Richter, pp. 67-78, 'Features in the electro-physiology of mental mechanisms'.

¹ Cf. Grey Walter, *op. cit.*

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2 *How Sense Data are Related to Physical Objects*

If each person is solipsistically confined in this way to his own sense data, which appear to him in his own private space, it is clear that we have to find out how each private microcosm is related to the public macrocosm ; how sense data are related to physical objects. Smythies says (pp. 27-28) that there are two possible theories about this. Either sense data have spatial relations with physical objects or they do not. Theory I is that sense data have no spatial relations with physical objects. This means that each person lives in his own three-dimensional private space, that there is a large number of these private spaces, but that these private spaces are not themselves arranged in any spatial order. Theory II is the more familiar view, popularised by Russell in *Mysticism and Logic*, of the arrangement of these numerous three-dimensional private spaces in a public three-dimensional space, the whole system being a six-dimensional one. There are two possible versions of Theory II. Theory II (a) says that there are 'unsensed psychical entities' beyond the confines of each person's private space, and Theory II (b) says that there are not. What I see when I look around me is, according to Smythies, my own sense data arranged in my own private space. And although my private space extends in principle indefinitely in all directions, in practice I have only a finite section of private space, limited by the furthestmost sense data which I am having. According to Theory II (a) my private sensed space is embedded like an air bubble in water in a larger unsensed space. This surrounding unsensed space is simply a continuation of the private space. It is the private space which I might have if I had sense data which were further and further away. Behind the private screen of sense data lie the unsensed psychical entities. Just as the works of the television set lie behind the television screen so these psychical entities lie literally behind the screen of sense data. The psychical entities are quite literally hidden by the sense data (p. 56). These entities are not in physical space : they are in the same three-dimensional system as sense data are, they bear the same relation to physical space as do sense data themselves. Theory II (b) says that there are no unsensed psychical entities. That there is nothing behind the private screen of sense data, that each private space ends at its furthestmost sense datum. But that the three-dimensional system which is a private space is itself in spatial relation with the three dimensional system which is physical space.

The experiments which will enable us to decide between these theories will consist, he says (p. 125), in detecting the *trans-dimensional* influences between the brain and the sense fields. He characterises how these trans-dimensional influences would appear to a physicist by saying that they would be, for him, forces acting upon the physical universe from the outside, whatever this may mean. He then says, but only says, that two different

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systems of mathematical physics would be appropriate to Theories I and II and that these *might* lead to different predictions as to the demonstrable effect of 'mind influences' on systems in the physical world. By 'mind influences' he means factors other than the laws of physics which (allegedly) operate in the brain.¹ The crucial matter of the precise way in which Theories I and II differ from each other empirically and the way in which this difference might be established is relegated to an Appendix of one and a half pages. The content of this Appendix turns out to be, not an indication of what these empirical differences might be, but a mere promise of such an indication.

3 Smythies's Case Against Naïve Realism

A case for a representative theory of perception is a case against naïve realism. If we see representatives of things then we do not see the things themselves. Yet the separate strands of Smythies's argument do not in fact add up to a refutation of naïve realism in quite the conclusive way that he thinks they do.

(i) For example, his method of introducing and defining sense data is neutral between a representative theory and naïve realism. It leaves things exactly as they are. It merely *redescribes* the original situation. It consists in saying that the *aspect* of a seen physical object which can be brought into spatial relation with an after-image is to be *called* a sense-datum. A naïve realist might very well reply 'Go ahead and call them that if you like!' It is absurd to think that this ceremony of renaming of itself gives us an entity, the sense datum, distinct from another entity, the physical object. Smythies speaks as if a change of attitude towards the contents of the visual field combined with a new terminology of itself justifies his saying that there are such things as sense data.

(ii) Since television must, logically must, be representative, and since vision resembles television, he argues (p. 42) that vision must be representative too. But this follows only if television resembles vision in the relevant respect, i.e. in being representative. The argument is circular. There is no trace of incompatibility in the idea that part of the causal story about vision should resemble part of the causal story about television, and that vision should be 'direct'.

(iii) He defends 'The Television Theory' against naïve realism by saying that 'it does not seem very plausible to assert that whereas physiological processes are necessary conditions for our perception of external physical objects, our perception of such objects is nevertheless a process in some manner different from the physiological process' (p. 42). This is an

¹ These ideas are taken over uncritically from Eccles's *The Neuro-Physiological Basis of Mind* (Chapter 8, *passim*.)

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extraordinary statement. He seems to be saying that the necessary conditions for perception must resemble the 'process' of perception. On these lines we could argue that since a heart in good working order is a necessary condition for seeing something that the 'process' of seeing something must resemble a heart.

(iv) The evidence from neurology, which Smythies takes as supporting his representative theory, does so only if the question is already begged. For this evidence is derived from two sources. (a) From clinical reports on the way in which the visual field is 'built up' during recovery from certain cerebral injuries (Chapter 2, *passim*). (b) From the patterns which appear in the visual field when the eye is exposed to a flickering light. Now it so happens that each of these kinds of evidence is just as good meat for a naïve realist as it is for representative theorists. The clinical reports on the recovery of vision are taken by naïve realists to be accounts of a gradually increasing access to physical objects. By representative theorists they are taken to be accounts of the gradual building up of the raw materials of vision into sense data. The clinical reports themselves are neutral between these two theories. In the same way, the patterns produced in the visual field by a flickering light are described by naïve realists as private visual sensations which have no more to do with the physical objects which are perceived in normal vision than have the odd effects which anyone can produce by screwing up and opening his eyes rapidly. These same effects are claimed by representative theorists to be the raw materials from which normal vision is built up. The facts themselves give us no way of deciding between these two accounts.

(v) The distinction between sense data and physical objects applies equally well, of course, in the special case of our own bodies. Here, too, we have direct access to a collection of somatic sense data which make up the 'body image' (Chapter 3, *passim*). We have to distinguish, that is, between what we experience of our own bodies (the 'body image') and what we know about them (the 'body concept'). In this connection Smythies cites phantom limbs as providing conclusive refutation of naïve realism. But the naïve realist who would be deterred by phantom limbs would have to be very naïve indeed. His view that we have direct access to physical objects would not be shaken by the fact that certain disorders give rise to persistent illusions.

4 What Kind of a Theory is a Theory of Perception ?

Smythies does not distinguish between philosophical and scientific theories. He seems to be unaware of the distinction between a philosophical theory which is going to apply to the facts whatever they may be, and a scientific theory which is, by contrast, verifiable, just because it will

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not fit the facts whatever they may be. Even to propose to give a verifiable version of a philosophical theory such as the sense-datum theory shows a complete misunderstanding of what such theories set out to do. To ask the question 'How are sense-data related to physical objects?' within the context of a scientific sense-datum theory is to ask for facts. To raise this same question within the context of a philosophical sense-datum theory is to ask for an elucidation of the relation between the *concept* of 'sense-datum', as defined in terms of incorrigibility, and the concept of 'physical object'. It is not to ask for facts, it is to ask, whatever the facts may be, about the relation between two concepts. It is clear that these two enquiries are different in *kind* and it should also be clear that the entities which are spoken of in both contexts as 'sense-data' are correspondingly different in kind. A scientific (Smythian) sense-datum is defined in the operational way indicated above. The question of the incorrigibility of such sense data is rightly nowhere raised by Smythies. A philosophical sense-datum, on the other hand, is defined essentially in terms of incorrigibility. The former is an empirical, the latter an epistemological entity. They have nothing in common except the name. They are introduced in different ways in answer to different questions. Philosophical sense-datum theories are about epistemological certainty. A scientific sense-datum theory could not and should not be concerned with this. Smythies is deceived by his calling the entities which he introduces 'sense-data' into thinking that they are the sense-data which philosophers have talked about. He is deceived by his own pun.

Smythies takes the philosopher to be a super-scientist who is concerned with the 'synopsis and synthesis of the various branches of knowledge'. If this were the case it would follow that the thing for all philosophers to do would be to learn as many sciences as they could and then set about synopsis-ing and synthesising them. This is not what philosophers do nor what they think they ought to do. There has only been one Herbert Spencer. Smythies says that 'an adequate and comprehensive account of perception' cannot be given in terms of an analysis of ordinary language. Since by an exhaustive account he means an account which includes all the facts known to all the relevant sciences this is obvious. But no-one has ever said that an analysis of ordinary language (whether this is in fact the proper business of philosophy or not) is a *substitute* for any of the sciences. The indiscriminate use of 'theory' in this way enables Smythies to reprimand philosophers for not being concerned with the detail of neuro-physiology, neurology, electronics, psychology, psychiatry, and the having of experiences under hallucinogenic drugs. But the complaint that a philosophical theory of perception is inadequate because it does not take into account the full details of half a dozen sciences can only be made if there is no difference in kind between a philosophical and a scientific theory. It never becomes

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clear in fact what Smythies thinks that a theory, either scientific or philosophical, is. This is one of the major confusions of the book. He says that he is not concerned with a choice between rival philosophical theories of perception. He contrasts them with his own comprehensive scientific theory of perception and assures us that this will take care of the philosophical puzzles about perception. Yet what he describes as philosophical puzzles, such as the question 'Where are sense data?' turn out to be quite specific questions which only arise within, and can only be solved within, the context of a particular philosophical theory, in this case, some version of the sense datum theory.

The book is intended to be a contribution both to the philosophy and the science of perception. Enough has been said to indicate that it is unilluminating on both of these aspects of the subject. The reader is promised a scientifically useful definition of 'sense datum' and a verifiable representative theory of perception. He is fed on promises which are never kept.

JOHN TUCKER

A HEGELIAN VIEW OF COMPLEMENTARITY

IN a nutshell, the thesis of the present volume¹ seems to be this. Kant was a determinist and Heisenberg was not. However, this is not to imply that Kant was wrong since *truth is relative*. A law, whether deterministic or antideterministic, is valid *only* within the 'mathematical framework' accepted at the time. This relativism, Cassirer seems to imply, is not an *ad hoc* attempt to save Kant, but a direct consequence of the necessary generalisation of Kantianism, intended to render it applicable to later developments in physics. The generalisation is necessary because of the invalidity of the alternatives to Kantianism, i.e. a theory of induction, and the view that theories are used for the economy of thought (positivism²). The 'only choice then is to generalise Kant's *apriorism* so as (a) to include the concept

¹ Ernst Cassirer, *Determinism and Indeterminism in Modern Physics, Historical and Systematic Studies of the Problem of Causality*. Translated by O. T. Benfey, with preface by H. Margenau. Yale University Press (London: Oxford University Press) 1957. Pp. xxiv+213. 40s. (The original edition was published in 1936.)

² Cassirer attacks Mach's Positivism. His own view is similar to Duhem's positivism. Cf. my 'Duhem versus Galileo', this *Journal*, 1957, 8, 237-248, esp. p. 245.

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of statistical laws in the Kantian concept of causality, and (b) to admit that the *a priori* 'mathematical framework' is, contrary to Kant's view, not fixed and ultimate, while insisting with Kant that it is a necessary condition of all scientific experience. This *relativist theory of truth* is Hegelian and the point of this review is to show that this is so in spite of Cassirer's professed Kantianism.

Cassirer's notoriously apologetic attitude towards Kant does not permit him to state a difference between Kant's and his own views as sharply as it is done here. Moreover, he uses his departure from Kant's views mainly as a defence of Kant, though admittedly in a very Pickwickian sense of defence. He claims (p. 123), that the law of causality still holds if it is formulated as 'if we can measure precisely the initial conditions of a system, then we can predict accurately its behaviour', but the antecedent, he points out, is, according to Heisenberg, never satisfied. Causality thus formulated holds vacuously. Cassirer evidently felt that this emptiness is far from being satisfactory. This is, of course, not to mention that we can formulate the law in a way which makes it false, e.g. 'the behaviour of any system is predictable, in principle, with any degree of precision'. He therefore also asserts that (although the world seems to us to be less well ordered than it seemed to Kant) at least the world is not entirely chaotic and that there is some kind of 'causality' even in statistical laws. However, Cassirer noticed the verbal character of his defence (viewed as successful in Margenau's preface, p. xi). Therefore, besides showing that Kant was partly right even by our own standards he asserted that even if Kant was wrong by today's standards he was right by yesterday's standards.

This however, in so far as it is acceptable, is quite trivial. We know that a Newtonian had, at least *prima facie*, some justification for being a determinist. The point is that since Newtonianism is not in agreement with facts determinism has no leg to stand on. Cassirer's assertion that the validity of a law is determined only relative to its 'framework' will not help, because we must consider the question of the truth, or validity, or acceptability, or what have you, of the 'framework' itself. To put it differently, why was the Newtonian 'mathematical framework' abandoned? Cassirer insisted that it was abandoned not because of lack of agreement with facts. We could always account for unpleasant facts, he argues, by *ad hoc* adjustment of minor points in our theory leaving intact the major ones, those which he called the 'mathematical framework'. Newtonianism was superseded, we are told, because of the greater simplicity of Einstein's theory. The demand for simplicity is an *a priori* demand, not an empirical fact. Yet, this argument does not help either. Quite generally whatever else is contained in our conditions for acceptability, if these are universal, they are ours as well as Kant's or Newton's. We know today that Newtonianism is unacceptable, because we know of facts which

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were not known in the days of Kant or Newton. Thus, even if we encumber our discussion by all kinds of sophisticated arguments (and these are not wanting), the fact remains that it was the discovery of new facts which somehow led us to reject Newtonianism. This may be illustrated by the situation in which two 'frameworks' explain facts adequately. In such a situation we try to design a *crucial experiment* which will enable us to overthrow¹ one of them.

Cassirer's theory does not explain even simpler cases such as the unacceptability of a theory identical with that of Newton in all detail save in its assumption about the number of the dimensions of space. In Cassirer's sense a four dimensional Newtonian 'mathematical framework' is a perfectly respectable 'mathematical framework', while of course no-one would have considered its acceptability. Admittedly, four dimensional Newtonianism is less simple than the ordinary three dimensional Newtonianism. This, according to Cassirer, is a minor defect. He cannot explain why Newtonianism was once accepted while four dimensional Newtonianism was not. The fact that a theory was once accepted is for him not something to be explained rationally, but something we must accept as given. He admits in the introduction to the present volume that in his *Substance and Function* (1910), 'classical physics was accepted as valid and uncontested' because at that time most physicists accepted it. One would expect him to have learned from experience and accept nothing 'as valid and uncontested', especially in a volume which propagates relativism. On the contrary, however, his relativism means that any view which enjoys universal agreement is true. Thus, he consistently accepts 'as valid and uncontested' quantum theory as he understands it, in spite of his view that quantum theory is inconsistent. (See Index, art. 'complementarity'.)

The assertion that Cassirer accepts the majority view 'as valid and uncontested' can be denied because while physicists usually consider quantum theory as consistent, he views it as inconsistent. This apparent disagreement with the majority view is the result of Cassirer's misinterpretation of Bohr's complementarity, or at least of Bohr's intentions. Cassirer suggests that Bohr's principle of complementarity asserts that quantum theory incorporates both the wave picture and the particle picture. It is because he so misunderstands the principle, and because he accepts anything that most scientists accept, that he is ready to justify Bohr—as he misinterprets him—by claiming that 'conflicts' are fruitful and need not lead to 'skeptical renunciations' (p. 174). Moreover, for this purpose he squeezes, even from Planck and Kant, permission for 'antitheses' in science (p. 80).

It goes without saying that Kant and Planck cannot be blamed. While 'light is both continuous and discontinuous' is plainly a contradiction,

¹ Cf. K. R. Popper, *Logic of Scientific Discovery*, London, 1958, Section 15, and his *Open Society*, third edition, London, 1957, vol. 2, pp. 12, 266, 364

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Kant's statement which Cassirer quotes concerns conflicting *tendencies*, and tendencies can contradict each other only in Hegel's dialectics, not in ordinary 'merely formal' logic. Planck, similarly, never asserted that since time reversibility and time irreversibility are incompatible properties the conjunction of Newton's dynamics (which is reversible) and classical thermodynamics (which is not) is a contradiction.¹ It must be pointed out that Bohr never claimed that quantum theory is inconsistent. He was worried about the inconsistency between two *intuitive presentations* of an abstract theory, and he wished to argue that even these two intuitive presentations do not clash because of Heisenberg's principle of indeterminacy. Thus, Cassirer had not the slightest reason to claim inconsistency of quantum theory on the authority of Bohr. Even if we grant, counter to Bohr's view, that the wave picture and the particle picture are in 'conflict', this will only imply the breakdown of an attempt to comprehend intuitively the (consistent) abstract theory. Yet Cassirer combines wave mechanics with the wave picture, and matrix mechanics with the particle picture, implying (e.g. p. 212, line 5 from bottom) that wave mechanics and matrix mechanics *contradict* each other because the statements 'electrons are waves' and 'electrons are particles' do. The fact is, however, that matrix mechanics is deducible from wave mechanics, and that the wave equation can be viewed as a means for transforming matrices into other (diagonal) matrices.²

Thus, Cassirer's reliance on Kant's, Planck's, and Bohr's authority is based on an error. None of these thinkers believed that contradictions in a theory need not lead to its rejection. The fruitfulness which Cassirer described

¹ Irreversibility of thermodynamics was pointed out as an argument against those who hoped to *reduce* every physical theory to Newton's theory of gravitation, or at least to a theory of Newtonian character, like, say, a theory of the form of a Hamiltonian equation. As Maxwell already pointed out, since all Hamiltonian equations are invariant to the transformation which reverses the time order, and since thermodynamics is not invariant to this transformation, one cannot deduce thermodynamics from any Hamiltonian. ('Maxwell's demon' is another argument for the same contention, namely that thermodynamics is irreducible to causal mechanics: while the demon violates thermodynamics he does not violate mechanics.) Planck's major fight was that thermodynamics should be accepted in its own right (see his *Scientific Autobiography*). In brief, irreversibility is an argument for logical independence, not for an 'antithesis', or a 'conflict', or a contradiction.

² Schroedinger claims to have shown that his wave equation is equivalent to matrix mechanics. The greater generality of his equation is shown by its applicability to a-periodical cases (which are not soluble by matrix algebra) like collisions and potential barriers. However, perhaps this is only evidence for the reviewer's reluctance to assert that the relation between wave and matrix mechanics is that of *mutual* deducibility. For the sake of the present review the assertion of compatibility between the two formal theories is enough.

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was not of self-contradictory theories but of free discussion between people whose opinions contradicted each other. In a free discussion one party tries to *refute* the view of the other party by finding a contradiction in it or between it and a statement of fact. If contradictions were accepted discussion would be futile since the refutation will not lead to a change of view. The fruitfulness of discussion is conditioned by our willingness to get rid of contradictory views, or of views contradicted by accepted observational reports¹. Cassirer approved not of conflict between ideas of two opponents, but of conflicts within the accepted 'framework'; that is to say, he approved of accepting inconsistent views. Of course it is his right to consider himself a Kantian; but one may argue that he was more of a Hegelian. Indeed, a Hegelian may accept determinism and indeterminism as two expressions of the human intellect in different periods, as parts of the *Zeitgeist*, as true each in its own *Zeit*. But a rational thinker should state what *arguments* made him change his views and defend those arguments—as Kant did. Whether Cassirer was a Hegelian or a Kantian, the present reviewer found in the whole volume not a trace of such a rational discussion of Cassirer's change of view from determinism to indeterminism. (However, it is odd that Hegel's name is never mentioned in a volume which so often seems to allude to him.)

Cassirer does not discuss his relativism and 'conflicts' in detail, but they recur in crucial passages, and especially in the decisive parts of the main sections of the book, section 7 on classical statistics, and section 9 on quantum theory, as well as at the end of the volume. The significance of these sections may be seen from the fact that Cassirer hardly discusses indeterminism anywhere else in the book whose title is *Determinism and Indeterminism in Modern Physics*.

Of the remaining eleven sections, one endorses the frequency interpretation of the probability formalism, two discuss the history of atomism, one discusses ethics², and seven are devoted to classical views. Readers of this *Journal* may expect to find in this book a thorough discussion of modern physics. It should be noticed, therefore, that the discussion of twentieth-century physics occupies a surprisingly small portion of the book. The major part of the present volume is historical. It contains too many

¹ K. R. Popper, 'What is Dialectic?' *Mind*, 49, 1940, p. 403 sqq.

² In his section on ethics, Cassirer shrugs off the statistical law in order to assert freedom just as Kant had shrugged off the deterministic law in order to assert freedom. Cassirer's act is perhaps less spectacular than Kant's, but it is at least more consistent. The reviewer considers this a great improvement, even from Kant's own point of view—assuming, of course, that Kant was greatly worried about his own inconsistency. But Cassirer views the situation differently, allowing for Kant's moral theory even in the presence of determinism. Here Cassirer is indeed consistent: Kant of the present volume does not allow himself to be worried about 'antitheses'.

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discussions for a short critical survey to go into. However, a few general remarks on them may be relevant. Firstly, they may be helpful for a research worker, for they are often very interesting; often containing almost unknown yet important information, and sometimes concern problems which are touched upon almost for the first time. Secondly, almost all the discussions are somewhat unreliable for the general reader for they are often much too brief to be valuable or even understandable. Only when Cassirer sticks to one problem for more than a page there is hope of seeing his point, though even the length of a page is not enough if the rich history of the 'union' between the calculus and physics is compressed into it (p. 161). It is doubtful whether many readers will flatter themselves that they understand Cassirer's occasional Greek, Latin, and French, and that they are sufficiently well informed to comprehend remarks which involve so many terms and ideas mentioned by Cassirer with little or no explanation. These include transcendental analysis (p. 17), Russell's theory of types (p. 30), Maxwell's demon (p. 77), Boltzmann's H-theorem (p. 79), 'energetic' thinking (p. 117), the Eleatic critique of becoming (p. 144), occult qualities (p. 146), stereochemistry (p. 147), transcendental logic (p. 166), Dedekind's definition of the irrational number (p. 169), and Wilson's photographs of the α - and β -rays (p. 182). Finally, apart from the dazzling effect of the frequent transitions from modern to ancient science and back, as well as between physics, mathematics, and philosophy of different periods, Cassirer hardly bothers to explain the purpose of his jumps, so that the volume lacks continuity.

In considering the volume as a piece of connected thought it is pertinent to ask: how much can the author get away with? Surely Cassirer did get away with the present volume, whereas Hegel did not get away with his notorious *Naturphilosophie*. Yet one may wonder how far from Hegelian confusion is the following not untypical quotation (p. 169): 'Cantor's foundation of the theory of sets and Dedekind's definition of irrational number could be considered as a final solution of the problem [of the continuum?], for *solely by means of pure thought a continuum was established which was truly commensurate with that of observable magnitudes and which was able to express the latter in all its individual features.*' (italics mine.) Each phrase in this quotation is a testimony to Cassirer's inadequate mathematical presentation while the quotation as a whole shows only his inadequate presentation of what is required from a solution of a mathematical problem. Cassirer admits almost explicitly that the discussion from which the above quotation is taken is a digression from his discussion of continuity in physics. Moreover, he discusses continuity in physics for no better reason than that waves are continuous and that he ought to comment on the wave-particle duality. Indeed, he ends the chapter on continuity with a jump into quantum theory, asserting that the

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'conflict' between continuous waves and discontinuous particles need not lead to 'skeptical renunciations'.

It seems clear why Cassirer could get away with such a confusion. He was a humanist and a universalist, he was interested in mathematics, in the social sciences and in the natural sciences, besides his being a Kant scholar, a historian of ideas, and an epistemologist. We revere the ideals of universalism and admire a person who shows some competence as a universalist, even if we admit in our hearts that in each field he will be beaten by any specialist. It is just because the present reviewer shares these sentiments that he is reviewing a book which it is hard to recommend. Our ideal should indeed be universalism—but not at any cost. If the result of attempts to achieve a more universal knowledge overlaps with Hegelian confusion then we should try a new solution to the problem of specialisation. However, we must refrain from taking the present volume as a model whatever views we may hold concerning other writings by Cassirer.

The volume is prefaced by an essay of Margenau which brings it up-to-date. Margenau seems to be much more informed about quantum theory, but there is hardly anything new or interesting in his preface, save his reference to the so-called von Neumann proof not as a proof but as 'very good arguments'. Indeed, Bohm's successful attempt to create a consistent theory which (a) asserts that electrons possess precise positions and precise momenta simultaneously, and which (b) contains Schroedinger's equation, seems to indicate that von Neumann's 'very good arguments' are not conclusive after all, that they do not consist of a proof that a theory which has these two characteristics must be inconsistent. (That Bohm's theory is *factually* false or at least very much *ad hoc* is, of course, entirely irrelevant, since the question is whether von Neumann's arguments consist of a proof that a theory like Bohm's must be *logically* false). Anyhow, it is a pity that Margenau only indicates a change of opinion, that he does not mention that prior to its actual occurrence Bohm's success was considered (at least by the majority of physicists) to be logically impossible. He should have attempted at least, to indicate what von Neumann's 'very good arguments' are, if not to explain and defend them.

However, Margenau is probably right in asserting that Bohm's theory would, if it be experimentally corroborated, only strengthen Cassirer's thesis (p. xix). What would refute Cassirer's thesis is a proof that classical physics already implied indeterminism. In his bibliography, Margenau mentions works by Popper and by Landé in which these philosophers claim to have argued very strongly for indeterminism on the basis of classical views only. But these works are not even alluded to in the up-to-date preface.

J. AGASSI

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Arthur Stanley Eddington. By A. Vibert Douglas.

Thomas Nelson and Sons, Edinburgh, 1956. Pp. xi + 207. 25s.

ONE day early in September 1939 I was talking to the late E. A. Milne in his study in North Oxford when he suddenly confided to me that he had recently bought a radio set and had been listening to broadcasts from Germany, as well as our own news bulletins. He then startled me by remarking that it had recently struck him that here was 'the voice of Eddington in the political sphere'! Conscious of my astonishment, he hastened to explain that all he was saying was that the object of these broadcasts was propaganda for a given point of view rather than the dissemination of truth. I went away marvelling at the superb parochialism of Cambridge men.

Despite an element of truth in his rival's peculiarly expressed charge, Eddington was one of the great men who, in the first four decades of the present century, caused many students of science to regard Trinity College as the centre of the universe. His pioneer work on the internal constitution of the stars is still generally regarded not only as his greatest contribution to knowledge but as placing him in the front rank of twentieth-century astronomers the whole world over. As Milne himself wrote shortly after Eddington's death, in this field 'it was Eddington who brought it all to life, infusing it with his sense of real physics', and of his supreme 'capacity to create interest to sweep away the cobwebs of formalism, to substitute real stars for mere "Gaskugeln" there can be and will be no two views; Eddington will always be our incomparable pioneer.'

Like Newton, however, Eddington was by no means a typical modern man of science, for he too was a voyager through strange seas of thought alone. Like Newton he was a synthesist and also a mystic. Even in the years when he was laying the foundations of theoretical astrophysics he did not lose sight of the grand objective—Fundamental Theory. The philosophy which Eddington associated with this theory has frequently been subjected to acute and damaging criticism, notably in the brilliant and witty lecture delivered in 1954 by Professor Dingle, but the symbolism of the theory and the extraordinary calculations of the constants of nature suggested by this symbolism are still capable of exercising a hypnotic fascination, despite frequently expressed charges of 'fudging' and 'numerology'.

Eddington's life and work therefore present a tremendous challenge to the biographer, and Miss Douglas must be congratulated on her courage. Even if the definitive assessment of the theory which Eddington regarded as the crowning achievement of his life has not, and probably cannot, yet be made, she has provided us with a simplified but absorbing account of his principal intellectual adventures.

I was pleased to see that her book is dedicated to my former mathematics master, C. J. A. Trimble, who was Eddington's most intimate friend for

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forty years. Mr Trimble had gathered much information about Eddington's family and his personal life which Miss Douglas gladly acknowledges. Eddington was a man of great reserve, and like many famous scientists lived a life of marked simplicity. This is one side of the picture, but intellectually his environment was that of many of the keenest minds of his age. Miss Douglas informs us that she has endeavoured to provide both portraits and 'the stereoscopic merging of the two into one faithful likeness of a great man'. It is no disparagement of her efforts to point out that one of the pleasantest features of the book which contributes significantly to this end is the beautiful selection of photographs, notably Eddington as a schoolboy, as Senior Wrangler in 1905, at the Kepler Monument in 1928 and finally after he had been elected O.M.

After three introductory biographical chapters taking the account down to Eddington's appointment as Plumian professor in 1913, there is a chapter on his work at Greenwich on star-streaming (discovered by Kapteyn in 1904) which culminated in his first book, *Stellar Movements and the Structure of the Universe*, published in 1914. In view of the philosophy of science which Eddington developed later, it is fitting that we should be reminded that he began as an observational astronomer.

A further biographical chapter is then followed by one on Eddington's contributions to Relativity. Eddington's interest in this subject—he was the first authoritative exponent in this country of Einstein's General Theory—was ultimately to be of far greater significance for him than his work in astronomy and astrophysics. For the development of his philosophy of science it was crucial, as Miss Douglas clearly indicates.

One of the most enthralling chapters in her book is that on Eddington's contributions to astrophysics, and her account of the Homeric debates with Jeans at the Royal Astronomical Society is fascinating. This is followed by further biographical chapters, including one on Eddington's religious beliefs, and then by a long chapter on Fundamental Theory. Miss Douglas has given us a fair summary of Eddington's ideas on this subject, even if in places it is somewhat superficial (perhaps unavoidably so). Her choice of quotation to head the chapter is particularly felicitous—four bars of Schubert's *Unfinished Symphony*.

A final word of praise must be given to the printer for the excellent way in which the book has been set up and to the publisher for making it possible for this absorbing biography to appear in print.

G. J. WHITROW

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Sound and Symbol. By V. Zuckermandl (translated by W. R. Trask).
Routledge and Kegan Paul, London, 1956. Pp. 399. 32s.

THE author's aim is to show that the study of music, though not providing fundamental information unavailable from other sources, gives useful clues for the elucidation of many basic philosophical problems, especially those of time and motion.

Physics depends on the primacy of vision and is concerned with visual-tactile space and spatialised time: time in music is pure 'duration' in Bergson's sense; and tonal space, though essential to music, is non-metric. Musical time, unlike physical time, is causative in itself. Tonal forces are objective but immaterial. 'Tone . . . is the only sensation that encounters us not as the property of a particular bodily-spatial thing. We see blue flower; we touch smooth wall; but we hear tone—not sounding string.' Gestalt psychology, so closely allied to Bergson's philosophy, has been preoccupied with visual, as opposed to aural gestalten because it has been afraid of St Augustine's old problem of the reality of past and future, which must be included in the temporal gestalt. What a pity that there is no tenseless form of the verb 'to exist': it would have saved the author so much argument!

While there can be no doubts about the interest of this book, one must reluctantly conclude that the author has not proved his theses. Where has he failed?

First, though Humpty Dumpty said, 'When I use a word, it means just what I choose it to mean; neither more nor less': yet it is not fair to give words several different meanings in the same argument. The words 'force', 'space', 'motion', 'time' and others are used with so many different connotations that the arguments become confused. To give a few illustrations only, for the use of 'force': 'Without forces, no bodies; but equally, without bodies, no force'; 'In the outer world there are forces active, whose activity transcends the physical'; 'We see forces in colours in the same sense in which we hear forces in musical tones'; 'Force is not an "operational concept"—something that we as thinkers add to the observed phenomenon in order to explain it . . . '.

Secondly, 'tone' (in his sense) is perceived in a way not so different from that of other sensations as the author believes. 'Music is motion, a continuous progression, and yet objectively, nothing but stases and gaps are given in it. . . .' This is necessarily mechanically true of most Western musical instruments, other than strings; but these are serious limitations and, as the author himself is clearly aware, the *portamento* of the human voice rebels against the rigidity of tones and semitones. The author believes that duration is truly perceived only in musical tones. 'To be sure, every sensation possesses duration; the pressure of the weight in my hand lasts

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while I hold it. . . . But here, the duration is nothing but a neutral base. . . . I do not touch duration in the heaviness.' Yet it has been shown that, if I squeeze an elastic body in one hand and, simultaneously, a viscous material in the other, my judgment as to which is 'firmer' will depend, within wide limits, on the time of the squeezing. Would Husserl (quoted by the author to support his distinction) have included such things as 'time objects'?

Thirdly, too many key experiments are neglected. To quote but a few: Hoagland, Piéron, and others, on the effect of body temperature on the sense of time; Piaget on space-perception; Michotte on perceived motion and apparent cause-and-effect; electro-encephalography in general; effects of mescaline on time-sense; and much work on the 'specious present'. Fourthly, many statements starting 'we hear . . .', 'we know . . .' are so subjective as to be meaningless, certainly to one not highly trained in Western music. Sound, as an air vibration, is more diffusely accepted than is light, but experiments with colour organs suggest that colour appreciation, as opposed to vision, may not bear a relation which is so very different in its mode of perception from that of tone, as opposed to the mere hearing of sounds.

Finally, the author is, in some places, extraordinarily naïve, e.g. How can God be 'timeless'? He would have to renounce music! Associationism, a very dead horse, is flogged with the wrong whip, and there is much confusion over 'the meaning of meaning'—of words and melodies.

Yet, in spite of its failures, this is a thought-provoking book which will make most of us realise how much more we (and the author) ought to read before we venture into some of these thorny fields.

G. W. SCOTT BLAIR

An Introduction to Scientific Research. By E. B. Wilson, Jr.

McGraw-Hill Book Company, 1952. Pp. xi + 375. £2 5s.

BOOKS on scientific method which might appeal to the research worker as a laboratory guide because of their immediate practicality are rare. It is interesting to reflect that most books on scientific method fall into two classes. The first usually tend to appeal (and this is unfortunate) more to the philosopher of science than to the practical scientist. Such books describe the processes of observation, induction, and deduction in logical terms, and treat science as an ordered system of statements whose truth values, relationships, and existential status are discussed. The second type of book frequently concentrates on the process of induction from a logico-mathematical standpoint and consists of a sophisticated attempt to justify the truth of statements arrived at inductively.

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Here is a book, several years old, which seems to have received little attention in this country. This is a pity, for it covers both fields mentioned and includes, as well, a much wider variety of topics of immediate use to the laboratory worker. The fields covered by the book range from the choice and statement of research problems to a final chapter on the reporting of the results of research. A useful chapter is devoted to reference books in various subjects and a list of abstracting, indexing, and review journals. This is followed by an account of scientific method. The account given is brief and Baconian. Perhaps the author will have an opportunity of revising this in the light of more recent publications on the hypothetico-deductive method if a new edition is ever contemplated. Other chapters cover the design of experiments, classification, sampling and measurement, the analysis of experimental data and the errors of measurement. Two essentially mathematical chapters on the technique of deduction and on aids to calculation are added. Each chapter concludes with a comprehensive bibliography.

There is little which is original in the subject-matter of the book, but for the most part, the references given are sound and the method of presentation is clear. Professor Wilson is to be congratulated in writing such an excellent reference work as are also the publishers for the excellent quality of the production.

R. J. F. WITHERS

Methods and Criteria of Reasoning. An Inquiry into the Structure of Controversy. By R. Crawshay-Williams.

Routledge and Kegan Paul, 1957. Pp. viii + 296. 32s.

In his new book Mr Crawshay-Williams brings before us a theoretical problem of great importance, the problem of how in a special class of cases 'we use language as an instrument of reason' (p. 3). His book derives value not only from the importance of the problem, which has suffered neglect for too long, but also from the detailed, careful, and orderly analytical treatment it receives. The sub-title which the book bears arouses interest, but as a description of the subject investigated, it is too general. For the investigation addresses itself, not to every type of controversy, but rather to a special, ill-understood type of disputation met with particularly in philosophy and in the more theoretical parts of the sciences. This kind of disputation he aptly characterises as 'intractable', a characterisation which might usefully have entered into the sub-title: *An Inquiry into the Structure of Intractable Controversy*. The style throughout the book, which is impressive for its wide range of material, is easy and engaging and often vivid.

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The author tells us that his 'original motive for the enquiry was a desire to find out why certain kinds of theoretical and philosophical controversy are so oddly intractable' (p. 3). Disputes such as those he cites, e.g. whether time is real, whether in the process of division the amoeba dies or only loses its identity, whether it is possible for a proper part of a whole to be as great as the whole, whether viruses are living things, and whether mathematical statements express thoughts, are disputes which apparently are or can easily become 'inherently inconclusive'. These may seem to many people to be like disputes over propositions for which there is as yet no conclusive evidence; but this likeness exists only in appearance, not in fact. Crawshaw-Williams makes the actual gulf between them visible at the outset by remarking the fact that in the case of the inconclusive disputes in philosophy and elsewhere argument continues without successful termination in the face of the stated evidence, whereas in the usual case of a disagreement over the truth-value of a proposition for which there is insufficient evidence further argument is considered futile and is suspended, or if the evidence is complete the argument comes to a successful conclusion. His own words are graphic and deserve quoting: 'The more purely scientific and factual the question involved, the more often this last is in fact what happens in case of disagreement; if the scientists have not enough evidence, they go out and look for more; they do not go on arguing. But the more theoretical and philosophical the question, the more often it is argued about interminably, even—or perhaps especially—by people who have been carefully trained in the technique of logical thought' (p. 3).

What is required both to come to an understanding of the nature of this important class of questions and to devise techniques for clearing them up is a precise and methodical investigation of methodology, the underlying idea being that this will bring to light factors the neglect of which causes fruitless debate and the specification of which yields results. The core theory developed in explanation of the nature of this class of questions roughly summarised is the following. The statements which the questions revolve on are of the kind that are not testable 'by appeal to definitions nor by appeal solely to the facts nor by appeal to the truths of logic' (p. 16) and the disputes to which they become subject are 'unfactual' (p. 15). The characteristic nature of such statements is indeterminacy with respect to context or purpose, and it is this indeterminacy which prevents the statements from being established and the controversies over them from being settled by show of fact. When these factors of indeterminacy are given attention and specified the indeterminacy vanishes and the statements become amenable to verification. In the writer's own words: 'If a question of the form "Is it correct to say that A is B?" is discussed in terms which exclude its being settled either by appeal solely to the facts or by appeal to logical deduction, then it will give rise to an inherently

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inconclusive controversy unless any answer put forward is related to a specific context or purpose' (p. 90). This thesis is worked out with a wealth of detail and is applied to recalcitrant problems in philosophy, biology, mathematics, physics, and to other fields. As a succinct application of the thesis, consider the question (p. 125): 'Is there no difference between a proposition of logic and a proposition of pure mathematics?' when asked and debated by people who know both logic and mathematics. This resists an answer which everyone will accept until it is explicitly related to a purpose: 'Is there a need to differentiate between a proposition of logic and one of pure mathematics?' Read in the context of purpose, i.e. in relation to the statement: 'There is no need to differentiate between logic and mathematics (or it is notationally uneconomical or inefficient or inelegant to do so)', the truth-value of the statement 'Mathematics is really logic' becomes accessible.

Crawshay-Williams's theory will undoubtedly have great appeal for many people, and especially for those who have become uneasy about philosophy, the great preserve of inherently inconclusive disputes. This reviewer, however, is doubtful of the correctness of the theory, for the following reason. The underlying premise throughout the book is that chronically debatable statements *have truth-values*; and the theory is designed to explain what it is which prevents agreement about the truth-values and how they are to be established successfully. But the truth-value of a statement is independent of the purpose of making the statement, and it is difficult to see how failing to make the purpose explicit could, by itself, possibly prevent the truth-value from being known. If the disagreement is *actually* about the truth-value then only more evidence, not mention of purpose, is relevant to the establishment or disestablishment of the statement; and if more evidence is of no avail, while mention of purpose serves to remove the disagreement, then the divergence cannot be about the truth-value of the statement. Crawshay-Williams maintains that his argument 'has at no time been that truth is' relative to purpose; it has been that the truth of an *indeterminate statement* is relative to purpose' (p. 255). But if an indeterminate statement *has* a truth-value, then it too must be independent of the purpose of the statement.

MORRIS LAZEROWITZ

Einführung in die symbolische Logik, mit besonderer Berücksichtigung ihrer Anwendungen. By Rudolf Carnap.

Springer-Verlag, Vienna, Pp. x + 209. 47s. 6d.

READERS of Professor Carnap's *Abriss der Logistik*, now long out of print, and all who are interested in the applications of mathematical logic, will

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welcome this book. But it is much more than a reissue of that earlier book. It is, as we are told on the dust-cover, 'a completely new work based on the present-day state of the science': we are also told that symbolic logic has become a basic science of the first rank, the importance of which is today highly estimated, especially in the Anglo-Saxon countries. This must refer to North America because it is hardly true of Great Britain. In this country it is held that logic is useless for science because Francis Bacon said so and, since we are a conservative people, suspicious of new things, it will require more than a century of De Morgan, Boole, Jevons, Venn, Whitehead and Russell (not to mention Pierce, Frege, Łukasiewicz, Tarski, the author of this book, and many others) to overcome the influence of the great Lord Chancellor. But let us try to forget the Lord Chancellor for a little while and look inside Carnap's beautifully produced book. It is divided into two parts. The first tells us all we need to know about symbolic logic in order to begin to use it in natural science. Everything is here explained very thoroughly, carefully, and clearly. The second part gives actual examples of applications in the form of axiom systems belonging to mathematics (set-theory, arithmetic, topology, and geometry), physics, and biology.

In Part I two symbolic languages are constructed: language A and language C. Strictly speaking these are not languages but skeletal frameworks of languages which become languages only when we assign an interpretation to certain of the symbols. In order to construct a language for dealing with a given domain of investigation we require, according to Carnap, (1) names for the objects which are regarded as individuals in this domain and (2) designations—called predicates—for the properties and relations which are exemplified in this domain. The language A contains the signs necessary for the construction of compound statements, e.g. negations, conjunctions, disjunctions, etc.; and when variables which can have statements substituted for them have been introduced, the principal formulas of the sentential calculus, which govern the manipulation of such compound statements, can be given. The next step is the introduction of signs which can have individual names substituted for them, so that, with quantifiers, expressions corresponding to the use of 'all' and 'some' can be constructed. Then predicate variables and the notion of higher order predicates are explained. These additions bring with them further lists of formulas involving their use. At this place (p. 38) Carnap explains the important distinction between intension and extension. A sentence has a proposition as its intension and truth-value (true or false) as its extension. An individual name has an individual concept as its intension, an individual object as its extension. For a one-termed predicate the intension is a property and the extension a class. Language A is completed by provision for the notions of identity, of cardinal number, and name-forming functors.

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The language C contains all the means of expression of language A with the exception of sentential variables, which are rarely needed for the formulation of scientific theories. It also contains linguistic devices which enable us to give briefer and clearer formulations of scientific statements. In this section we learn to operate with predicates, and to construct statements involving compound predicates, without the use of individual variables. No distinction is made between a predicate as a name of a property and the same sign as a class designation (languages A and C are purely extensional), and so no sign of class-membership is used (p. 96). This part also contains an admirable account of relation-theory, together with sections on the λ -operator, descriptions (which are not much used), finite and infinite sets, and continuity. Between the parts devoted to languages A and C is a part called 'Language System B' in which are given the syntactical and semantical rules for languages A and C, and such topics as definition, proof and derivation, and the system of types, are explained.

Part II, in addition to giving examples of axiom systems belonging to various sciences, also contains expositions of the distinction between thing-languages and co-ordinate languages and their various forms, quantitative concepts and the formulation of laws, and several pages devoted to a general explanation of the axiomatic method.

All sections of the book are provided with illustrative examples of the methods discussed and exercises for the reader. There are copious bibliographies and indexes. An enormous amount of useful information has been packed into these 209 pages. Of all recent books on mathematical logic this is by far the most convenient from the point of view of the student of science. From the point of view of the philosopher of science, the axiomatised theories here presented offer valuable material for investigation. The following misprints have been noticed: P. 9, line 12, 'schein' seems to be a misprint for 'sein'; p. 82 last line but one, a subscript 'j' appears to be missing after a Gothic S; p. 114, line 20, the letter 'x' in the quantifier should be 'v'; p. 127, line 8, 'x' seems to be a misprint for 'a'. An English translation of this book is in preparation.

J. H. WOODGER

The Background of Astronomy. By Henry C. King.

Watts, London, 1957. Pp. vi + 254. 18s.

ONLY during the last two or three centuries have the sciences begun to break free from irrelevant presuppositions and to develop autonomously. In the earlier phases of their growth they cannot be studied profitably in isolation from one another or from the doctrines which contemporary

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philosophy imposed upon them. Particularly is this true of astronomy so closely connected in its origins with astrology, alchemy, medicine, mathematics, and other disciplines, and so circumscribed by arbitrary schemes of cosmology.

The task of sketching the early growth of astronomy to the beginning of the seventeenth century in its context of contemporary philosophy and religion, science, and superstition, has now been undertaken by Dr H. C. King, Senior Lecturer in Ophthalmic Optics at Northampton Polytechnic, London, and the author of a standard work on the history of the telescope.

The story opens with some account of the planetary deities and the astronomer-priests of ancient Mesopotamia and Egypt (chs. i and ii). The progress of Greek astronomical thought and technique is next followed from Ionia and the Italy of Pythagoras to Athens and thence to Alexandria and to temporary extinction in medieval superstitions (chs. iii-vii). The fortunes of the Greek scientific heritage are then bound up for some 800 years with the circuitous transmission of Moslem culture (ch. viii), until in the twelfth century the tradition crosses the frontiers of Western Christendom. Following two chapters (ix and x) on the 'gratifying illusions' of anthropocentric cosmology and on medieval enterprises in navigation, cartography, and time-measurement, we reach the fifteenth-century break with the imitative traditions of scholasticism (ch. xi); thereafter the time-honoured ideas of the old astronomy are progressively exploded by Copernicus, Kepler, and Galileo, and the way prepared for the establishment of the modern synthesis (chs. xii and xiii).

Dr King's book does not claim to represent the fruit of extensive original research, being, as he avows, 'derived largely from second-hand sources'. However, he has collected much interesting and significant material from a wide range of reading (his Bibliography comprises 164 titles) and has given us a useful companion volume to more formal presentations of the development of theoretical astronomy. Early man, in the author's view, utilised the vicissitudes of the heavenly bodies to measure the progress of time. 'Yet this temporal need was by no means the only spur to astronomical observation. No less important was his desire to worship something greater than himself, for this found its expression in early fertility cults which often involved particular reference to the moon and later to the entire host of heaven' (p. 2).

We have noted but few slips in this carefully written book. In the reference to the planets on page 36 (l. 9) the word 'major' should read 'superior'. On page 84 (l. 6) 'Cronon' should read 'Conon'. On page 206 (l. 22) 'Ptolemy' should apparently read 'Copernicus'. And the first of the two books attributed to O. Neugebauer on page 245 is the work of A. Neuburger.

A. ARMITAGE

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Reflections of a Physicist. By P. W. Bridgman.

Philosophical Library, New York, 1955. Pp. xvi + 576. \$6.00

THIS book will be welcomed by students of the philosophy of science as it collects thirty-two previously scattered essays by the leader of the once powerful operational philosophy. The papers were published between 1929 and 1954, and they fall into the following headings: operationalism, applications of operationalism to specific scientific problems (mostly physical), sociology of science, and ethics of science. They are all excellently written and, with but a few exceptions, they are accessible to the general scientific reader.

The revolutions in the physics of our century have, like those in the eighteenth century, stimulated the framing of philosophical theories depending on some of the various possible interpretations of certain physical theories. Operationism was one of these philosophies: it had both the advantage of being up to date, and the disadvantage of going the way of all things present—namely, of turning into past. The wide appeal of operationism in the thirties and even in the forties can be understood not only by recalling the prestige of pragmatism in the United States, but also the state of physics itself, which seems to us to have been a lot less sophisticated than it is today. At that time few physicists dared playing with arbitrary cut-offs, or with divergent series, or with Lagrangians involving no 'operationally defined' quantities. Fictions and artificial tricks were thought to belong to classical physics. The new physics knew only 'natural' procedures and, through philosophical autosuggestion, it ended by believing that it employed only symbols designating measurable quantities. Things, properties, and relations that were not defined in terms of possible laboratory operations simply did not exist. (For example, 'A body has position only in so far as its position can be measured', p. 176.) In those days of shrewd innocence relativity used to be explained with the help of an infinite staff of observers filling the universe. This was done on the belief that reference systems, to be physically meaningful, had to have human operators—even if infinite and fictitious, and even if these could not actually observe any Lorentz 'contraction' of lengths. Expositions of the quantum theory often started with Heisenberg's imaginary X-ray microscope for the observation of electrons as if one could dispense with highly elaborate constructions, such as Hamilton's (or Hamilton-Jacobi's) formulation of dynamics. It was also fashionable to state that special relativity had sprung from an epistemological analysis of the simultaneity concept. This was declared physically meaningless even though relativity calculates time differences between inertial systems (which may or may not be inhabited by observers), thereby assigning to simultaneity a non-operational meaning. It was also fashionable to assert that quantum mechanics had

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emerged from an operationist examination of the measurement process, although the quantum theory of measurement was sketched much later (and is still in its infancy), and although ψ was anything but an operationally defined symbol. (It is interesting to note that, unlike many of his followers, Bridgman himself recognised that the operationist requirement had no creative virtue of its own but played only a suggestive rôle in leading to one of the many possible solutions.)

Today, after the enthusiasm for the pragmatic approach to non-pragmatic activities has waned, operationism might be classified as a school of semantics abiding by the empiricist principle that only statements about observations and measurements are meaningful and, consequently, capable of being true or false. Now the theory that 'meanings are operational' (as Bridgman himself describes it) and that scientific concepts must be defined in terms of laboratory operations (to which 'pencil and paper operations' were later on added) is open to at least the following objections. First, both good and bad science contain statements about facts, statements about observation or measurement of facts, and statements about symbols (both with regard to their mutual relations and to their meaning). If only operationally meaningful terms were kept this sandwich would degenerate into a single layer and we would be prevented from realising that observations are observations of something and that many statements are not informative about operations but about formal properties of symbols and about their connotation. Second, science is full of unobserved entities and non-measurable, or at least only indirectly measurable, qualities such as quantity of electricity, energy, potential, entropy, wave phase, adaptation, organisation, organic integration, and so on; they are not, however, occult qualities, but are clearly related to experience by means of theoretical constructions. If we sacrificed all such concepts in the name of some philosophy, we should get rid of the upper, most abstract levels of science, those in which the deepest and most general relations are grasped.

Third, one and the same 'quantity' may be assigned various operational definitions. Thus, according to Bridgman, we must distinguish between a tactual and an optical length. Moreover, some quantities, especially in the most advanced chapters of science, can be defined both operationally and by way of what Margenau calls constitutive definitions, i.e. in terms of further constructs. In the case of temperature if it is defined as that which we 'read' in thermometers (without any underlying theory of heat?) we obtain as many 'definitions' as kinds of thermometers. Even worse, since such 'definitions' presuppose the attainment of an equilibrium state, what then of non-equilibrium situations, which are the most interesting of all? Should we describe them without the benefit of employing the temperature concept just because its operational definition fails for transient phenomena? Is it not one of the advantages of scientific theories that they are

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able to transcend the limitations of the empirical level and that they unify the variety of experience by recognising, for example, that all kinds of thermometers serve the same basic purpose? This only requires us to distinguish qualities and relations from both their intensity and the techniques of their measurement.

Fourth, if meanings are operational, since factual truth is dependent not on form but on meaning, there should be, as Bridgman points out, as many different kinds of truth as techniques employed in establishing the validity of synthetic propositions. (And why not in mathematics as well? Would it not be consistent with operationism to state that there are as many Pythagorean theorems as techniques of demonstration—that is, various dozens?) These different truths would be incommensurable: thus, two different numerical results obtained in the measurement of the length of a stick, one with 'tactual' and the other with 'visual' techniques, would be equally valid. An experimentalist would not be entitled to value one technique higher than another if they relied on different senses.

Fifth, if meanings depend only upon operations, and if operations are, according to Bridgman, essentially personal, then 'we' should end up in solipsism or should at least surrender the communicability characterising (or so we had been told) science as against mystical visions. There is, indeed, an unbridgeable chasm of meaning between you and me, just because our operations are different; as a result, 'science is essentially private'. If so, why care about methodological uniformity—and why be sure that it is possible at all? Sixth, if not only human acts but everything else as well should be analysed into doings (operations) rather than into objects or processes, since operations are private and historically changing, we must conclude that 'nature is intrinsically and in its elements neither understandable nor subject to law' (p. 169). Irrationalism is thus offered as a consolation for the solitude of solipsism: *Thou shalt build the world for thyself, but thou shalt not understand thine own work.*

True, Bridgman retreated early enough from the extreme position he had adopted in *The Logic of Modern Physics* (1927) by granting that 'paper and pencil operations' may be used besides laboratory procedures. They must, if only because the design and actual performance of measurements (not to speak of the building of the apparatus and instruments) requires a host of hypotheses, among them the law statements about the phenomena involved. But then, 'operational definitions' are not a radical starting point but rather, when desirable and possible at all, an intermediate link in a long chain of doings guided by theory about objective facts, human doings, and formal entities. How could such a liberal interpretation of operationism be reconciled with the reduction of concepts to percepts, with the programme of levelling all language strata to the sense-data language (supposing one was spoken among civilised men).

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It is conceivable that operationism—and, in general, pragmatism—could have been fruitful in the social rather than in the physical sciences, not on account of its dislike for speculation (since sociology suffers mostly from poverty of theoretical models) but because of its emphasis on the analysis of phenomena into human doings—which is obviously legitimate (but, then, trivial) in the case of human phenomena. However, in the hands of Bridgman, operationist sociology is just a naïve revival of eighteenth-century social atomism—hence not a science of society at all.

It was important to insist on the need of relating even the remotest scientific (non-formal) terms to experiment but this has been done untiringly since the birth of modern science. It has been less important, and definitely crippling, to strive for the elimination of those terms which are not 'close enough to facts'. It has been equally mistaken to believe that the connection between the various levels of scientific discourse, from sense-data to the most elaborate theories, should amount to a reduction or levelling down. Besides, such (unsuccessful) attempts to reduce scientific theory to the quantitative expression of 'actual facts' had been inaugurated by Mach (whose philosophy Bridgman does not even mention). Indeed, it was Mach¹ who first tried to keep in physics only 'measurable physical attributes', rejecting as 'superfluous and futile' whatever was not reducible to the 'elements' (i.e. the sense-impressions). One wonders why Mach is not credited with the founding of operationism (even granting the *charme* of such 'paper and pencil operations' consisting in the rediscovery of ideas which have been known of old). And one wonders, too, to what level science would have fallen had it been faithful to the operationist programme. General relativity, for example would not have been invented, since it was rejected by Bridgman as un-operational—and rightly so.

Operationism has been an important episode in the philosophy of modern science, and Bridgman's essays are indispensable to its correct appraisal.

MARIO BUNGE

Concepts of Space: The History of Theories of Space in Physics.

By Max Jammer.

Harvard University Press, Cambridge, Mass., 1954. Pp. xviii + 196.

\$3.75

THIS is a systematic and highly stimulating history of the concept of physical space by a scholar who is at home with mathematics, physics, philosophy, theology, and classical languages—a highly exceptional combination indeed. The book's contents are: Foreword by Albert Einstein and Introduction;

¹ See, e.g. E. Mach, *The Science of Mechanics*, transl. from the 9th edn. by T. J. McCormack, London and La Salle, Ill., 1942, pp. 598 ff.

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(1) The concept of space in Antiquity ; (2) Judeo-Christian ideas about space ; (3) The emancipation of the space concept from Aristotelianism ; (4) The concept of absolute space ; (5) The concept of space in modern science. Philosophers of science will probably be most interested in the last chapter (pp. 125-190).

Like every historical monograph this one assumes principles of historical method and some ideas about the main stages of historical development. The method here employed is the tracing of intellectual influences of certain writers on others, with disregard of the cultural atmosphere in which they lived. The author's basic tenets about the history of culture seem to be : first, that everything worth mentioning began in Greece, and second, that Jewish theological and metaphysical speculation was instrumental in shaping modern science. Thus, pre-classical Antiquity and Indian thought are left behind, and the history of the space concept up to modern times is largely reduced to a strife between Aristotelian and Jewish ideas. Moreover, the thesis is advanced that the Newtonian concept of space—that absolute void functioning as God's sensorium—is entirely or primarily due to Hebrew thought. In support of this latter thesis, Professor Jammer exerts his ingenuity in devising a causal chain starting from eminent Rabbis in the Hellenistic period and culminating in Crescas (c. 1400), the Catalan Jewish schoolman who criticised Aristotelianism from the viewpoint of Hebrew theology. Not the Greek atomists, who are barely mentioned, not Giordano Bruno, but Crescas is credited with presenting the West with the opinion that physical space is void, homogeneous, isotropic, and infinite ; moreover, Crescas' thought would have influenced Newton's world view even more than his own physical research, the revival of atomism, the geographical discoveries, and Renaissance pantheism. Much in the same vein, the fugacious revival of optics during the thirteenth century is regarded by Jammer as a mere result of the neo-Platonic thesis that space is light and God's emanation. But is this consistent with the decline of neo-Platonism just at that time ? And even assuming that such a renaissance was nothing but a miraculous consequence of neo-Platonic writings why did these same writings lack such a power in Byzantium ? The reviewer's main objection to this kind of history of ideas is that every theory is thereby regarded as functioning in a cultural vacuum. Actual life, the epoch's interests and strivings, its *Zeitgeist* are ignored. Thinkers are treated like bookworms obtaining their whole nourishment from written paper, which nourishment barely enables them to produce further paper to feed further worms. In such a way it is not explained why author A_n was influenced precisely by author A_{n-1} and succeeded in influencing, in his turn, author A_{n+1} .

Although it is one of the author's merits to have compressed a wealth of material in a few pages, some omissions are worth while correcting in a future edition. For example, Euclid's ideas on physical space and on

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geometry (which he conceived as the science of the ultimate nature of matter); Augustine's thesis that space and time are inseparable from matter and change; Oresme's reinvention or rediscovery of co-ordinate geometry; and the contemporary idea that microphysical space is a fluctuating jelly ($ds = \gamma_\mu dx^\mu$). On the other hand, some feebly grounded speculations like those of the 'new' cosmology should not be treated on a par with theories that have stood some empirical test.

There are also, of course, quite a few debatable assertions. Thus, Plato is credited with the geometrisation of physics. How could he if he denied the very possibility of building a science of nature? Besides, Aristotle's cosmos was not thoroughly ordered: the sublunar sphere, man's home, was chaotic and corruptible. And how could Al-Ghazali be regarded as the typical representative of Muslim philosophy unless it were previously demonstrated that there has been a sufficiently homogeneous system of 'Muslim philosophy'? Also, has recent criticism not discredited Duhem's rash assertion that Philoponos' *impetus* theory was the starting point of modern dynamics (instead of being an unsuccessful attempt to patch up peripatetic physics)? Further, why should Mach, who rejected the relativity theory, continue to be regarded as a forerunner of relativity, and not as a thinker whose criticism of Newtonian mechanics contributed to defrost the attitude towards dynamics? Finally, it is controvertible whether logical positivism can be credited with the recognition of the physical implications of modern theories of space. Is not the structure of physical space according to most positivists (including Jammer) 'a function of our conceptual scheme'? Partisans of relativity, on the other hand, tend to think, as Einstein himself recalls it in his Foreword to this book, that 'there is no space without a field' and that space is not independent of matter (whether in its particle or in its field form). Moreover, if special relativity reinstated Leibniz's relational theory of space (by regarding spacetime as a system of objective relations among point events), general relativity has superimposed on this relational structure the Cartesian concept of space as a thing (by welding space and gravitational field to such a point that a field-free region is not regarded as being *in* space).

Despite many controvertible points, Jammer's book is a *must* in its field, not only because of its impressive amount of information, but mainly because it is anything but a catalogue of ideas and books. It is a serious examination of the problems, scientific, philosophical and theological, clustered around the space category. The inner logic of theories (but unfortunately not their cultural background) is discussed with deepness and clarity. The present reviewer therefore hopes that Jammer's book will elicit what his philosophy of history seems to preclude: namely, the emergence of new ideas.

MARIO BUNGE

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Les Fondements logiques des mathématiques. By E. W. Beth.
Gauthier-Villars, Paris, 1955. Pp. xv + 241.

THIS is the second edition, revised and enlarged, of a book that first appeared in 1950. It is presented as an introduction to the methods used in the foundations of mathematics (p. xi), a survey of contemporary views in this subject, and an explanation of their philosophical consequences (p. 5).

The range of topics covered is very large. We are offered summary accounts of mathematical logic (propositional calculus and functional calculus), set theory, the customary recursive definitions of real and rational numbers in terms of integers, an outline of syntax and semantics (with special emphasis on methods developed by Gödel and Tarski), the Frege-Russell reduction of arithmetic to logic, the views of intuitionism, an extensive discussion of the logical antinomies and much else. Most of the important work of the past century is at least mentioned, and there are scattered references to investigations no more than five years old.

The 'philosophical consequences' are briefly discussed in a preliminary chapter on Aristotle's conception of science, and in a concluding section. The author's view is, briefly, that western philosophy and science have been dominated by a rationalistic ideal first authoritatively stated by Aristotle. It is the pattern of Euclid's *Elements*—an organised body of knowledge, arranged in the form of theorems connected by chains of proofs with explicitly formulated axioms, themselves incapable of proof. Traditional rationalism, still powerful today, invokes logical and metaphysical principles, held to be self-evident, indemonstrable, and certain. Now the development of the philosophy of mathematics (and especially the discovery of the logico-mathematical paradoxes) has forced a radical revision of the supposedly self-evident principles of logic and mathematics, though not in any fashion that commands universal agreement. It is not clear what moral is intended to be drawn, unless it is that it is harder than ever to be a rationalist in these days.

The subject of the 'foundations of mathematics', largely ignoring the philosophical interests of its pioneers, has become a vast domain of highly technical studies, in which the non-specialist badly needs the orientation which an introductory survey might be expected to provide. The outsider need only look at G. Kreisel's stimulating paper, partly intended for 'the general reader' (this *Journal*, 4, 107-129), to appreciate the difficulties of access. And the original papers are still more forbidding, as a glance at any volume of the *Journal of Symbolic Logic* will confirm.

Yet it is possible to provide the kind of help that is needed. Among the examples that might be cited are Russell's *Introduction to Mathematical Philosophy*, the introductory chapters of *Principia Mathematica*, W. V. Quine's essay on Whitehead (in *The Philosophy of Alfred North Whitehead*, Evanston,

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1941), Alfred Tarski's 'The Semantic Conception of Truth' (*Philosophy and Phenomenological Research*, 4, 1944, 341-375) and Raymond L. Wilder's *Introduction to the Foundations of Mathematics* (New York and London, 1952). The kind of 'popularisation' achieved in these surveys demands very special gifts—among them a capacity to disentangle the 'point' of a mathematical or logical proof without pretending to reproduce the full argument or blurring the line between what can be understood immediately and what has to be taken on trust. The reader must get immediate insight, in full awareness of the extent of his ignorance and the measures needed to repair it.

It would be welcome news if the book under discussion had met these exacting standards. Such results as the various Gödel theorems, the Löwenheim-Skolem theorem, modern investigations on models, and the stimulating ideas of the intuitionists deserve to be widely understood. But this book cannot be recommended for non-specialists: its explanations are often obscure, sometimes inaccurate, and hardly ever successful in communicating 'grasp' or 'insight'. A beginner would be bewildered by finding trivialities jostling remarks pre-supposing an elaborate mathematical education; and the expert must be repeatedly irritated by vagueness or imprecision. It is hard to forgive a writer who gives references by citing only the author's name and the year of publication. The bibliography provided is inadequate.

The book reads like a set of notes to be used in connection with a course of lectures. It would need very thorough revision and expansion to fulfil the task to which it addresses itself.

MAX BLACK

Prediction Methods in Relation to Borstal Training. By Hermann Mannheim and L. T. Wilkins.

H. M. Stationery Office, London, 1955. Pp. vi + 276. 17s. 6d.

THIS is a highly competent piece of work. It sets out to make a contribution to methodology—not in the sense found in philosophy of science but in the natural sense of that word; that is to say, it is a study of methods of doing something such as playing golf or playing chess—in this case predicting proportions of successes and failures among the boys (whose technical name is 'lads') selected for Borstal. The authors, imposing certain conditions on themselves—the method must be simple, efficient, repeatable, and 'valid'—hope to provide something that is usable by the Courts and that will be an improvement on previous prediction methods. In their aims they appear to be entirely successful; and it is pleasant to find statistics used in this field for sensible purposes. It makes, subject to one important reserve to be mentioned later, an important contribution.

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The problem is this. A boy is considered for Borstal. A decision has to be taken between two possibilities—putting him in Borstal or not doing so. If put in, then on release he will either get into further trouble and return to Borstal or he will not. If he returns he is a 'failure', otherwise a 'success'. How can we predict of possible candidates for Borstal which ones are likely to be successes and which ones failures in this sense?

There are two general ways of setting about this: compile all conceivably relevant factors (such as drunkenness, broken home, other delinquents in family); score existing Borstal boys for these factors; and one can then obtain a table and scores for possible Borstal boys. The other method, which is followed by the authors, is simpler: weight the factors; omit those that statistically do not influence the result; and one can arrive at a table more simply and quickly. It then has to be validated for other samples of boys.

The authors call this a prediction table, warning us that it is in fact something else—an 'experience' table, used for prediction. A better name might be 'situation-table' or 'situation-index'. Further developments are possible. This index can separate definite sheep from clear-cut goats; but there is a nondescript residue. Still, if the foregoing method is one of rough tuning, another for fine tuning can be utilised for the residue.

As might be expected, these methods predict better than do medical reports, assessments by prison officials, etc. An unexpected result is this. The fact that 'open' Borstals are significantly more successful than 'closed' ones might be attributed to their being allotted boys who are better material and therefore a better risk. But by means of statistical procedures the authors have been able to show that, while this is in part the case, it is not sufficient to explain the greater success of the open Borstal, and that this must be credited with a greater reforming influence.

I turn now from the content to the form of enquiry.

The authors labour somewhat at the justification of their way of working. Thus from the fact that their method can predict successfully in seven cases out of eight some critic is supposed to say that therefore one case is incorrectly classified, and the authors explain that this is a mistake. It is hard to understand how anyone who is capable of reading this book seriously could make this mistake. Conceivably this and other comments are intended for dynamic psychologists who think that, because work like the present has nothing to do with the inner lives of the boys, therefore it cannot be of any use at all.

The schism—investigating unconscious conflict versus investigating other matters—is a curious one. An insurance company can tell what proportion of people is likely to die by the age of forty-one without knowing anything about their health or medical history. Likewise it is reasonable to try to calculate how many will be recidivists without knowing anything about their mental make-up.

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On the other hand it does not follow that, because a statistical inference of this kind is valid, there is therefore nothing for the dynamic psychologist to add. The authors make it plain that there is something—and it is especially interesting to see what this is. After the table or index, with rough and fine tuning, has yielded all it is capable of, the result may be, say, that you have isolated a large group of boys with a 7 to 1 chance of success if they go to Borstal—but you don't know which are which (in this connection it is interesting that the rough tuner predicts on the basis of information about past criminal career, whereas the fine tuner does so on the basis of personal qualities). To try to find out which are which one could use interviews or methods of dynamic psychology (subject to a certain proviso). This means that, though neither interviewing in general nor psychiatric investigation in particular is uniformly satisfactory for prediction of recidivism, the interviewer or the psychiatrist could be on very strong ground if, after making his prognoses and classifying them as 'successes' or 'failures', he then modified them to make them fit the proper statistical proportions—a procedure often adopted by psychologists and by examiners in distributing marks.

The proviso stipulated by the authors is that the interview should utilise factors other than those used to form the statistical tables (otherwise it would not be independent of them but merely a repetition and a poor one). This condition might or might not be fulfilled if the interviewers were well-meaning people without special training; it would almost certainly be fulfilled if the interviews were conducted by clinical psychologists, for such an interviewer would be using as factors the impact of the boy on himself in certain specific ways, the boy's particular way of putting things, and his mode of responding. Thus the proviso may be fulfilled in practice. But the authors have surely made a serious theoretical mistake in supposing that there is any need to fulfil the proviso at all. Take the set of factors required to form the index and give them without any other information whatever to a clinician, and he will bring to bear on them a background of experience about their significance and how they can fit together. The authors' assumption would be true only if their statistical procedure could squeeze all possible information out of the given factors—and this no one could claim.

This leads to a further interesting point made in the book. It is held, with regard to 'intangible' factors, such as those a clinical psychologist deals with, that they are amenable to scientific tests provided a form of words can be agreed that makes the intangibles communicable. This is arresting, mystifying, and would have been worth elaborating.

To sum up the methodological position: A new type of statistical procedure is being developed, which shows considerable promise of yielding fairly accurate predictions. That such a procedure is possible is of

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theoretical interest, especially as the factors used in predicting are not causal. The method of the present book has likenesses to forecasting in meteorology. You have a thermometer, a barometer, and so on. Changes of temperature, pressure, humidity, and cloudiness are not necessarily causes of change in the weather; but whether causal or not they are indices of the change. A clear example of a good predictive index would be the clinical signs of rising temperature and falling pulse; yet neither would cause the ensuing state of the patient—the causes would be quite different. In other words, this book provides a dashboard with a number of dials which enable you to know where you were and are and where you are likely to get to. That is why I have suggested that 'index' would be a good name for the tables, for it would convey clearly both their rôle and value and also their limitations; that is to say they will tell you what may become of a Borstal boy, but they tell you nothing about him, and of course can be no guide about what is a wise way of handling him. The 'intangibles' despite their intangibility become indispensable at this point. Despite the limited scope of the method, it is a triumph of statistics to be able to use so successfully such relatively superficial quantities as indices. Their practical importance is that they can be used when the actual causal factors are too complex to unravel; their theoretical interest is that they can function in this way at all, seeing that they are not linked in any simple way to the causal factors.

A final word on book-making. The authors bring out the complementary rôle of this statistical work and of clinical psychology but do not bring it out strongly enough—readers might easily, though wrongly, get the impression that the methods described in this book were sufficient (or the main ones) in deciding questions about delinquents, and busy Home Office officials, whose policy may be shaped by this work, might well fall victim to this impression—which could become serious. A point of architecture: the admirable history of the subject is, as is most often the case, in the wrong place as chapter I, where it tends to put one off. The time for history is after the problem has been outlined—then its interest becomes obvious at once. A small point is the repeated reference by the authors to an equation that is never given. Finally could the publishers not produce the book in a manner more worthy of their patron? Such paper and lay-out put one off reading it—after all, this is a Government production that was probably meant to be read.

J. O. WISDOM

Factor T. By Stefan Themerson.

Gaberbocchus Press, London, 1956. Pp. 68. 6s.

THIS is a brilliant little essay, evidently written by a kind of mind that is all too rare. Factor T is the tragedy that results from Needs plus Dislike of

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what is needed—when it is vitally necessary to do what one vitally dislikes. This is shown in the Dislike of tomatoes, without which life could not be supported, among a tribe in Notwahrocteh. A negative form of the factor T at the moment of writing is that this book is outside the sphere of reviews in this *Journal*, yet it is one that especially philosophers of science should enjoy. Or does the author's adventure with the problem of induction alter matters? The solution offered is not so striking but it abounds in excellent sidelights.

The quality of the writing is delightful, the author clear and cogent in logic and psychology. He deals faithfully with the inference :

- Only an elephant or a whale gives birth to a creature whose weight is 70 kilogrammes or more ; the President's weight is 75 kilogrammes ; therefore the President's mother was either an elephant or a whale.

And the essay may be said to contain a fundamental theory of psychology having affinities with Freud and Plato. These are good chromosomes to be endowed with, especially when grown in a Lewis Carroll environment. The book is excellently illustrated by Franciska Themerson.

J. O. WISDOM

La Théorie harmonique. Vol. I : *Le Principe de simplicité dans les mathématiques et dans les sciences physiques* ; Vol. II : *Biologie*. By André Lamouche. Paris, Gauthier-Villars, 1955, 1956. Pp. 481 + 575. 3,000 fr.

THIS monumental work is the product of thirty years of study in the history and methods of the sciences on the part of a well-known French engineer. The reviewer cannot claim to have fully digested all the arguments which are here presented with great lucidity, subtlety, and scholarship, but wishes to call attention to these volumes and to the author whose *Méthode générale des sciences pures et appliquées* (Gauthier-Villars, 1924) received little notice in the English world.

The *Harmonic Theory* is a comprehensive neo-Pythagorean interpretation of the characteristics both of natural thought and of rational processes, having as one of its ultimate aims a strengthened harmony of thought and action. The interpretation is based on a *principle of simplicity* which is applied to mathematics, physics, and biology. Under this principle the parallel development of complex from simpler logical structures, and of complex from simpler natural phenomena, involves chains of steps determined by a *rule of maximum relative simplicity*. The master concept of *simplicity* is subjected to a close analysis, and the ultimate exemplar of simplicity is found in the integral numbers. It is for this reason that Lamouche's philosophy of

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an underlying harmony between thought processes and other natural phenomena has been called 'a true pan-Pythagoreanism'.

Much reading and thought have gone to make this eloquent salute to a persuasive simplicity, and insight is displayed in the avoidance of some current errors. For example, following Planck, *a priori* metaphysics and radical positivism are treated as twin enemies to the growing seeds of thought. This openness towards new developments may result from the fact that the author's intention is to discover a principle which is so deeply implicit in the use of reason as necessarily to cover simultaneously all applied logic and all scientific theories.

This high generality can be a source of weakness. One suspects that all facts which can be conceived and rationally ordered could in some manner be read as supporting the author's doctrine. In that case the doctrine may be an intuitive anticipation of one feature of a future scientific philosophy covering all phenomena, including thought. But if that is so, then the author (like all of us) still lacks certain indispensable insights which could enable his discursive treatment to be converted into a concise and unmistakably authentic and objective statement of valid principles underlying all systematic knowledge.

It is impossible here to discuss the author's arguments in detail. The volume on physics is concerned with such topics as simplicity, rhythm, unified fields, complexity, discontinuity, and determinism; the volume on biology with chance and finality, unity and diversity, levels of organisation, selectivity, specificity, and exploits the conceptions of the finality and 'inventiveness' of organic processes which have been stressed by Cuenot. There is a bibliography, but no index.

One section is headed 'Aimez qu'on vous critique'. This encourages the following comments. If a Pythagorean philosophy is sufficient for science, why are so many basic problems still obscure? Can asymmetrical relations ever be adequately treated by a doctrine based primarily on equivalences? If a principle of simplicity is all-comprehensive and already correctly understood, why are a thousand pages necessary?

But every sensitive reader must admire an author with so many talents.

L. L. WHYTE

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The British Journal for the Philosophy of Science

VOLUME IX

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ON DIMENSIONALITY AND CONTINUITY OF PHYSICAL SPACE AND TIME *

B. ABRAMENKO

I *Introduction*

'Consider, for example, the words on this page. To a casual glance they form a two-dimensional distribution; but they were written in the hope that the reader would regard them as a one-dimensional distribution.'

A. Eddington, *Space, Time and Gravitation*

RECENTLY an article was published in this *Journal*, which investigated an interesting problem of the causes for three-dimensionality of space.¹ The author, however, left without consideration the question: does space actually have three dimensions? Although the affirmative answer to this question seems to be self-evident, it is worthwhile to subject the whole problem to a detailed analysis and to clear up the meaning of the statement that our physical space is three-dimensional and that the world, time included, is four-dimensional.

As was pointed out by Eddington² this 'is not so clear as it appears at first. An aggregate of a large number of things has in itself no particular number of dimensions. In order to define the number of dimensions we have to postulate some ordering relation. This relation appears to be the interval.' He demonstrated convincingly the importance of the conventional assumption of a particular ordering relation by an example, cited above, of dimensionality of the words printed on a page.

It will be the task of this article to try to detect also other conventions, which explicitly or implicitly were made, in defining the number of dimensions of our physical space and time.

* Received 28. vi. 57.

¹ G. J. Whitrow, this *Journal*, 1955, 6, 13

² A. Eddington, *Space, Time and Gravitation*, Cambridge, 1953, p. 186

2 Criteria of Three-dimensionality of Space

‘Vom Wesen des Raumes bleibt dem Mathematiker nur die eine Wahrheit in Haenden: dass er ein dreidimensionales Kontinuum ist.’

H. Weyl, *Mathematische Analyse des Raum-problems*

Various criteria of three-dimensionality of space have been put forward at different times. They can be divided into three main classes according to their way of approaching the problem: geometrical, analytical, and physical. In what follows we shall consider and analyse them in this sequence, which may somewhat differ from the chronological order of their origination.

A. GEOMETRICAL CRITERIA

(i) *Triad of orthogonal axes.* The fact that only three mutually perpendicular axes can be constructed at any point of physical space was probably the oldest criterion of three-dimensional space structure. However, this circumstance does not prove the three-dimensionality of space, because a question can naturally be raised: just why perpendicular lines should be preferred above all others so that they could define the number of dimensions? If we choose some other angle between the axes, e.g. 45° , the number of axes inclined to each other through this angle would be 13. This number can determine the number of dimensions just as little as number 3 in the triad of orthogonal axes, unless some decisive arguments can be advanced as to exceptional and superior status of right angles over all other values of angles, apart from convenience or tradition.

In nature, right angles have no preferential status; side-by-side with the cubic crystallographic system, exist also hexagonal, trigonal, tetragonal, rhombic, monoclinic and triclinic systems with very different angles between crystal surfaces and axes. Therefore, the concept of spatial three-dimensionality cannot be based upon the triad of orthogonal axes, unless the preferential status of right angles is assumed by convention, by virtue of their convenience.

(ii) *Criterion of the number of parameters.* At first, this criterion was formulated so that the number of parameters necessary and sufficient to locate any point in space determined the number of dimensions. So, Exner¹ wrote concerning this definition: ‘immer aber sind drei

¹ F. Exner, *Vorlesungen ueber die physikalischen Grundlagen der Naturwissenschaften*, Vienna, 1919, p. 11. The author is indebted to a referee for pointing out this book

voneinander unabhaengige Bestimmungsstuecke erforderlich und das ist das charakteristische Moment, weshalb wir von einem dreidimensionalen Raum sprechen.' Later on, when Peano¹ showed the possibility of accomplishing this with a lesser number of parameters, Brouwer² re-formulated the criterion so that dimensional number should be defined as the number of parameters, by which a manifold in the vicinity of any of its points can be uniquely and continuously represented. He based this definition on the invariance of dimension number.

Both of these definitions rest upon an implicit assumption of the continuous structure of space. At first sight, the relation between continuity and dimensionality might not be obvious. Yet, it will be shown below that a close connection exists between them, and the problem of dimensionality can be solved only after adopting some particular structure of space with respect to its continuity.

(iii) *Recursive definitions of dimensionality.* Poincaré³ determined dimensionality of an n -continuum by means of dividing it by sections of $(n - 1)$ dimensions, and considered the latter as 'walls' limiting the extension of an n -dimensional body or a part of it. By recursion he came to the point, which was assigned zero-dimensionality, without however giving a rigorous definition to the very conception of 'dimension'. The similar recursive definition by Menger⁴ involves only a slight change in formulation and substituted 'boundaries' (Begrenzungen) in place of Poincaré's walls in arbitrarily small vicinities (Umgebungen) of any point.

Both of these recursive definitions deal explicitly with continuum, as was stressed by Poincaré, and assigning three dimensions to space on the basis of these definitions means adoption of the continuous structure of space from the outset.

B. ANALYTICAL CRITERION

An attempt to employ an analytical method only, rather than purely geometrical ones, in considering the problem of space

which contains much important material on the problems of space, time and dimensions, in particular in lectures 3, 9 and 15. Another relevant publication, also kindly pointed out by a referee is the book by P. Frank, *Das Kausalgesetz und seine Grenzen*, Vienna, 1932, p. 23

¹ G. Peano, *Math. Ann.*, 1890, 36, 157

² L. Brouwer, *Journ. f. reine und angew. Math.*, 1912, 142, 146

³ H. Poincaré, *Dernières Pensées*, Paris, 1913, p. 67

⁴ K. Menger, *Dimensionstheorie*, Leipzig, 1928, p. 79

dimensionality was undertaken by Eddington.¹ He introduced a so-called *E*-frame in description of physical phenomena and deduced the three-dimensionality of space from its properties consisting in the fact that no more than three perpendicular directions connected by circular rotations are possible in the world possessing pentadic group structure. Hence he concluded: 'Space defined as a domain in which the relativity rotations are circular, is restricted to three dimensions'.

However, the properties of the same pentad indicate the existence of two more dimensions connected by hyperbolic rotations with the other three, which apparently point out at two-dimensionality of time. Eddington himself rejected this conclusion, calling the fifth co-ordinate a phase, a time-analogue, but not the second dimension of time, which is, in his words, 'entirely beyond experience'.

Thus, apart from conventionality of adopting this *E*-frame, acceptance of three-dimensionality of space and rejection of two-dimensionality of time, following from the same frame, cannot be recognised as self-consistent.

C. PHYSICAL CRITERIA

(i) *The form of the known physical laws.* Whitrow considers the form of the inverse-square law of gravitation as a decisive criterion of three-dimensionality of space, for its expression in an n -dimensional space looks otherwise, namely as follows:

$$F = \kappa \frac{Mm}{r^{n-1}} \quad (1)$$

The universal validity of the inverse-square law for gravitational forces is, however, very questionable in the realm of cosmic distances. The spiral structure of galaxies casts serious doubts on it. Although numerous attempts have been undertaken to account for this structure within the framework of the conventional gravitational law, some additional, more or less arbitrary assumptions were postulated which stretched explanations and made them artificial. On the other hand, theories involving plausible modifications of the Newtonian law, as e.g. Vogt's theory,² explain the spiral structure quite naturally. Thus the inverse-square law of gravitation is limited, from the cosmical scale

¹ A. Eddington, *Fundamental Theory*, Cambridge, 1953, p. 124

² H. Vogt, *Die Spiralnebel*, Heidelberg, 1946, p. 156

viewpoint, to insignificant distances. These are, so-to-speak, spatial limits. Besides these, temporal limits may exist, requiring some changes with the passage of time. Such a varying law of gravitation appears in the theories of Milne¹ and Jordan.² The latter adduced much evidence in favour of it.

Thus, being limited spatially and temporally, the inverse-square law of gravitation paraphrasing Whitrow³ himself: 'can be usefully applied to a wide, but not universal, range of natural phenomena'; it cannot be considered as universally valid and proving the three-dimensionality of space.

Moreover, gravitational forces are not the only forces in nature. Turning to nuclear forces we do not find there the inverse-square law in operation. As these are the most powerful forces known, it is not clear just why the criterion of dimensionality should not be based on the form of the nuclear force potential, if an approach from the standpoint of physical forces is to be employed. Then, one would arrive, on the basis of Formula 1, at a quite unexpected number of dimensions, by far in excess of three.

Therefore, the argumentation based on the form of the physical laws can even be turned against the three-dimensionality of space, and in any case cannot be used as positive evidence for this point.

(ii) *The form of a law combining gravitational and electromagnetic fields.* In 1918, Weyl⁴ developed a theory which was intended to bring about unification of the metrical gravitational field, characterised by the fundamental tensor g_{ik} , with the electromagnetic field characterised by the vector ψ_i . Analysing his results, Weyl⁵ came to a conclusion that the unification leading to the simple and harmonic form of electromagnetic laws established by Maxwell was possible only in a four-dimensional world. Reserving one dimension for time, Weyl came finally to the postulation of three-dimensionality of space as an unquestionable truth, as cited above.

The subsequent development of science has not confirmed Weyl's unified theory, and his argumentation for three-dimensionality of space, based upon it, has lost its power.

¹ E. Milne, *Mon. Not.*, 1944, 104, 120

² P. Jordan, *Schwerkraft und Weltall*, Brunswick, 1952, p. 191

³ G. Whitrow, this *Journal*, 1951, 2, p. 58

⁴ H. Weyl, *Sitzber. Preuss. Akad. Wiss.*, 1918, 1, 465

⁵ H. Weyl, *Philosophie d. Mathematik und Wissenschaft*, Munich, 1927, p. 98

3 *Dependence of Space Dimensionality on Continuity*

Λόγον ἔχειν πρὸς ἀλλήλα μεγέθη λέγεται ἃ δύναται
πολλαπλασιαζόμενα ἀλλήλων ὑπερέχειν

Euclid, Fifth Book

Some of the criteria mentioned above depend explicitly or implicitly on the assumption of the continuous space structure and validity of the Archimedean axiom as formulated, for instance, by Euclid.

(i) *Continuity of Space as an Axiom*

‘Qu’est-ce qu’un point de l’espace ? Tout le monde croit le savoir, mais c’est une illusion.’

Poincaré, *La Science et l’Hypothèse*

Does the continuity of space present an empirical fact or an *a priori* property, the truth of which is self-evident ? Neither. There is no empirical evidence whatsoever to this effect, but some facts, to be considered below, point out to the contrary.

That the notion of continuity is not above suspicion, shows the whole history of philosophy from the time of Plato up to the present: controversies between Aristotelian adherents of continuity and their adversaries have never subsided. And in modern philosophy Whitehead¹ expressed this quite unequivocally: ‘The continuity of space apparently rests upon sheer assumption unsupported by any *a priori* or experimental grounds’.

Mathematicians, even those who contributed most to the development of abstract ideas of geometrical continuity as represented by arithmetical continuity of real numbers, also recognised the possibility of different space structure, as was admitted by Dedekind:² ‘Hat ueberhaupt der Raum eine reale Existenz, so braucht er doch nicht nothwendig stetig zu sein.’ So the status of the continuity notion as a postulate ought to be constantly borne in mind.

With this is closely connected the Archimedean axiom, usually formulated that for any 2 quantities ‘*a*’ and ‘*b*’ there always exists an ‘*n*’ such that $na > b$. If this is re-written as follows: $\frac{b}{n} < a$, the alternative equivalent formulation, as applied to geometrical entities, would sound as follows: for any two fragments of a straight line, ‘*b*’

¹ A. Whitehead, *Essays in Science and Philosophy*, New York, 1947, p. 281

² R. Dedekind, *Stetigkeit und irrationale Zahlen*, Brunswick, 1892, p. 12. In this and other quotations from the old German publications, their original obsolete orthography is preserved

and 'a', however small 'a' may be, there always exists an 'n' such that 'b' being divided by it will be shorter than 'a', approaching zero, i.e. a mathematical point, as $n \rightarrow \infty$. The acceptance of validity of the Archimedean axiom in this form is equivalent to the recognition of mathematical points as meaningful entities in physical space.

But what is a point, asked Poincaré, and quite justly answered that an alleged knowledge of it is nothing but illusion. If definition of the mathematical point as an abstraction does not involve any difficulties, to define a point in our physical space is an extremely hard problem. In its essence, the point in space is an inverse of infinite space, just as zero is an inverse of mathematical infinity. This correlation between two infinities, in the large and small, was very pointedly emphasised by Weyl:¹ 'Der Raum ist nicht nur in dem Sinne unendlich, dass man in ihm nirgendwo an ein Ende kommt; sondern an jeder Stelle ist er nach innen hinein unendlich, ein Punkt lässt sich nur durch einen ins Unendliche fortschreitenden Teilungsprozess fixieren.' Such an infinite dividing process, although thinkable abstractly, could hardly be applicable to physical space. At any rate, its applicability, i.e. validity of the Archimedean axiom in nature, is in just the same need of experimental confirmation as any other postulate of geometry, as was admitted by Hilbert.²

We postpone to Section 4 (iii), the discussion of physical data pertaining to the structure of space, because they should be considered together with some time properties, which are the subject of Section 4 (i), (ii), and explore now the alternative discontinuous structure of space, in so far as this has a bearing on the problem of its dimensionality.

(ii) Discrete Structure of Space

Unter 'Punkt', 'Gerade' usw. sind in der axiomatischen Geometrie nur inhaltsleere Begriffsschemata zu verstehen. Was ihnen Inhalt gibt, gehoert nicht zur Mathematik.

A. Einstein, *Geometrie und Erfahrung*

Adoption of discrete structure means that we drop the postulate of continuity, as defined by Hilbert³ or Dedekind, and the postulate of Archimedes. We mention both of them, for they are independent of each other, and some non-Archimedean geometries were actually

¹ H. Weyl, *Philosophie d. Mathematik und Wissenschaft*, Munich, 1927, p. 33

² D. Hilbert, *Math. Annal.*, 1918, 78, 405

³ D. Hilbert, *Grundlagen der Geometrie*, Leipzig, 1913, 4th edn., p. 21

devised in which continuity was preserved, as those of Veronese, Hilbert, etc.¹

To renounce these postulates means the following: the location of any physical point in space can be determined only with a finite accuracy down to a certain minimum value l_0 ; distances of a lesser value have no physical sense. The physical space cannot be subdivided beyond a certain limit. These ideas were extensively developed by March.²

We shall now apply this conception to our problem of space dimensionality. Let us consider first the location of a point in a plane. For this purpose, let us draw on this plane an Archimedean spiral

$$\rho = l_0 \theta \quad (1)$$

with separation of two neighbouring windings being l_0 (Fig. 1). Any

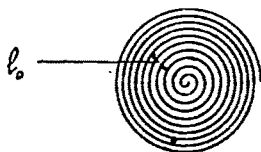


FIG. 1.

point on the plane can be located by the length of the spiral arc from the origin to this point, expressed by the formula:

$$L = \frac{l_0}{2} \left[\theta \sqrt{1 + \theta^2} + \text{Ar sinh } \theta \right] \quad (2)$$

There is no sense of speaking about the points situated between any two windings of the spiral, since they are indistinguishable from the points lying on the windings themselves, because their separation is limited by l_0 . Therefore, this spiral covers the entire area of the plane, as if the spiral had windings which possess a very small width l_0 and adhere closely to each other so that no spacing is left between them. The location of any physical point on the plane is determined uniquely and continuously (in a sense that no abrupt transitions occur) by a single parameter L .

Now extend this conception to space.

Let us draw this spiral on a plane and then roll the plane into a similar spiral surface with the same spacing of windings equal to l_0 .

¹ F. Enriques, *Enz. Math. Wiss.*, Leipzig, 1907, 3, I, 1, p. 117

² A. March, *Zeit. f. Phys.*, 1937, 104, 93, 161; 105, 620; 106, 49, 291

(Fig. 2). If the curve were of infinite length, the resulting spatial spiral will fill the whole of space for the same reason as above: the points between the windings of this surface are indistinguishable from the points in the surface itself. Thus in this case, too, the location of any physical point in space is uniquely and continuously determined by a single parameter, the length of the curve from the origin.

A possible objection can be raised against this procedure, namely, that our plane must be rolled, as otherwise no extension of it could produce space. It is true, but rolling does not affect dimensionality. A cylindrical surface obtained from a plane does not differ from the latter either metrically or dimensionally. In our case also there is no difference in dimensional respect between the flat and rolled surfaces.

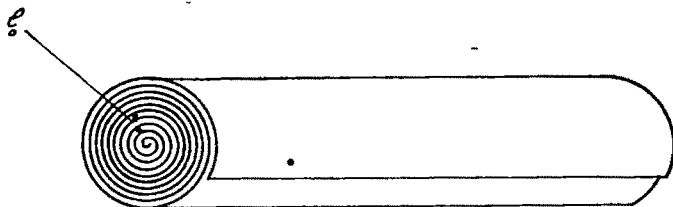


FIG. 2.

(iii) *How many Dimensions has Discrete Space ?*

‘ ein diskontinuierlicher kompakter Raum ist nulldimensional ’.

K. Menger, *Dimensionstheorie*

If we wished now to apply the criteria listed above to this discrete space model, we would suffer failure. It is easy to see that neither Brouwer's criterion of parameter number nor Poincaré's and Menger's recursive definitions are applicable to discrete space.

Indeed, from this possibility of representing it by one parameter, are we entitled to conclude that space is one-dimensional ? Not at all. This would be the case, if the spiral itself were a continuous line. However, by adopting discontinuous structure of space, we should consider the spiral line as also being discrete, which is composed of physical points separated by the same least length l_0 . In this case, both the spiral line and the space generated by it may be expressed in terms of the number of physical points composing them. Such a system, consisting of discrete points, will not be a continuum any longer; it will be called a ‘ zero-dimensional cell system ’ and denoted by Z^0 .

As such, this system will be the subject of combinatory rather than continuous Analysis situs.

Newman,¹ in his development of combinatory Analysis situs, used the theorem of superposition as an indicator for the topological equivalence of complexes. If we apply the criterion of superposition to our spirals, we shall note the following: a spiral with widely separated windings, pictured in Fig. 3 (conventionally called one-dimensional), can be superposed upon the closely-wound spiral covering the entire area of a plane as described above, shown in Fig. 1; a plane rolled with spacings, pictured in Fig. 4 (conventionally called

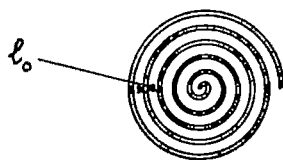


FIG. 3.

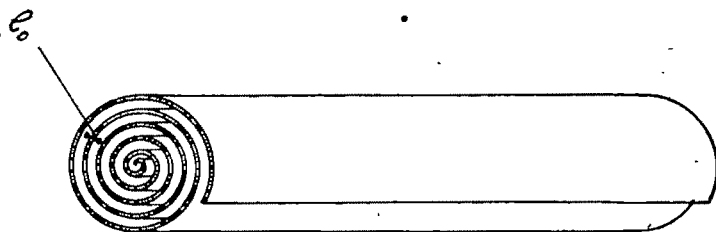


FIG. 4.

two-dimensional), can be superposed upon the closely-packed surface filling the whole space, shown in Fig. 2. Therefore, all these sets are topologically equivalent, although they would have different numbers of dimensions, had they been continuous manifolds.

Another criterion, 'Verwandschaft', was proposed by Tietze:² two Z^n are 'verwandt', if they possess an equal number of points. Applying this criterion to our system described above, we can always compare spirals of equal length and consequently containing the same number of points. This property, Verwandschaft, is sufficient for homeomorphy of zero-dimensional complexes, which indicates their topological equivalence.

¹ M. Newman, *Math. Ann.*, 1928, **99**, 399

² H. Tietze, L. Vietoris, *Enz. Math. Wiss.*, Leipzig, 1930, **3**, 1, 2, p. 207

PHYSICAL SPACE AND TIME .

The concept of dimensionality, in the conventional meaning, loses all sense, for it is not applicable to discrete spaces. Thus, we have shown that the problem of dimensionality of our physical space cannot and should not be solved before and independently from the problem of continuity.

We have analysed several criteria proposed for the establishment of a certain number; i.e. number '3', as the number of space dimensions, and we have seen that all rest upon some explicit or implicit assumptions or conventions, in addition to Eddington's 'ordering relation' or interval. These criteria were the following:

1. Preferential status of orthogonal axes to all others,
2. Continuous structure of space and significance of geometrical points as physical points (Archimedean axiom),
3. Adoption of Eddington's pentadic group structure,
4. Unrestricted validity of the inverse-square law of gravitation,
5. Validity of Weyl's unification of metrical and electromagnetic fields.

Only when one or several of these assumptions are postulated, can the physical space be considered '3'-dimensional. Thus, the conventional character of this statement becomes obvious. The axiom of the three-dimensional nature of space is neither an *a priori* truth, nor an experimental fact, and so it does not differ in this respect from all other geometrical axioms, which by Poincaré's definition:¹ 'ne sont donc ni des jugements synthétiques à priori ni des faits expérimentaux. Ce sont des conventions.'

The number '3' of spatial dimensions is not an absolute number, nor is it intrinsic in the very nature of space. If it were so, this number would inevitably manifest itself in some dimensionless constant of nature, as this was already recognised by Ehrenfest.² No such connection between the number '3' and any of the universal dimensionless constants has ever been proved.

Therefore, the final conclusion of our investigation can be formulated as follows: the three-dimensionality of space is not its absolute characteristic, but a property conventionally ascribed to space in view of assuming tacitly at least one of the above-mentioned axioms, in particular the axiom of continuity. Thus the treatment of the problem, as was pointed out by a referee, basically pivots on the idea that the number of dimensions ascribed to space depends upon some theory.

¹ H. Poincaré, *La Science et l'Hypothèse*, Paris, 1920, p. 106

² P. Ehrenfest, *Annal. d. Phys.*, 4 Folge, 1920, 61, 440

4 *Some Problems in Topology of Time*

'What we, as scientists, know about time itself is very little indeed. . . . Time moves, or we move through it, in one direction only.'

G. Clemence, *Time and Its Measurement*

The sacred awe which implacable time inspires in us is the main reason preventing our learning more of its nature and properties. Perhaps the Clemence view that time study constitutes the subject of philosophy rather than science but contributes to maintaining this aureola of transcendentalism enveloping time. This viewpoint seems to have become obsolete since the ushering in of relativity, which dethroned *χρόνος* and deprived him of his mystic halo of being absolute.

(i) *Space-Time Relationship*

Gurnemanz: 'Du siehst, mein Sohn, zum Raum wird hier die Zeit.'

R. Wagner, *Parsifal*, 1st act

Any discussion of space properties would be essentially incomplete, if space-time relationship were not taken into account. Both of them together constitute an inseparable framework of our physical Universe, and space in itself 'sinks to a mere shadow', using the famous Minkowski expression.¹

Therefore, we expand the scope of this essay to consider some problems of space-time relationship and dimensionality of time, but not considering for the present the problems of time nature and its unidirectional sense of flow.

First of all, one fundamental question has to be answered: whether the union of space and time has any physical meaning, or does it merely represent a convenient mathematical tool possessing no more physical significance than time-temperature or time-pressure combinations in weather diagrams. The latter view is often maintained, and it is sometimes stated that 'space and time have almost nothing in common, beyond their interchangeability in certain equations'.² If this viewpoint were based on a firm foundation, the space-time union would be deprived of any physical meaning, the properties of one of them would have no connection whatsoever with the properties of

¹ H. Minkowski, *Phys. Zeit.*, 1909, 10, 104

² G. Clemence, *Amer. Scientist*, 1952, 40, 260

the other, and changes affecting one of them could occur without necessarily simultaneously affecting the other.

There exist many facts and phenomena which indicate the opposite. A close physical relationship between them is manifested first of all in the existence of the universal constant 'C' having dimension of ST^{-1} , which was stressed by Heisenberg,¹ although its significance is not as yet fully understood. The entire edifice of special relativity rests upon this physical relationship of space and time. One particular feature is worthy of emphasis at this point, for it will be important in the further discussion of this problem.

The world line and time-axis of a system moving at velocity v with respect to another system of reference will constitute an angle to the time-axis of the latter:

$$\psi = i Ar \tanh \frac{v}{c} \quad (3)$$

This turnability of time-axis is of great fundamental importance. It means that any system has theoretically not only a single line in time direction, which must be unavoidably pursued, but a whole infinity of them restricted only by 'isotropic' lines, with which they constitute an angle equal to ∞ .²

In this respect, the time property is similar to that of space, in which infinity of motion directions is possible. It can be argued that this similarity is only superficial, and the motion through time is not as easy as the motion through space, for 'We can manipulate spatial separation in a way impossible with time, which cannot be juggled with',³ for time is irreversible and it is impossible to come back to the same point in time, whereas one can readily return to the same point in space. Actually, however, this distinction is only apparent. The identification of one spatial point at two different time instants is just as little possible, as the identification of one temporal point in two different points of space. As the simultaneity in two widely separated points cannot be defined because of the non-existence of absolute time, so the identity of space points in two time instants cannot be determined because of the non-existence of absolute space. Thus, in this respect, the properties of space and time are also similar.

¹ W. Heisenberg, *Das Plancksche Wirkungsquantum*, Berlin, 1945, p. 13

² F. Klein, *Vorlesungen ueber nicht-euklidische Geometrie*, Berlin, 1928, p. 143

³ M. Johnson, *Amer. Scientist*, 1951, 39, 412

Both space and time are liable to distortion by a gravitational field, as can be seen by inspecting Schwarzschild's solution¹ of general relativity equations, and by the presence of electric charges, as shown in Jordan's solution.²

Thus any factor, be it the state of motion, acceleration, gravitational field or electric charges, etc., which affects space, also affects time, and in this respect they are also alike.

All this strengthens the view, that first; the union of space and time is not merely their mathematical amalgamation in some formulæ, but a physically significant close interrelation, and second; that their properties are so strikingly similar in many respects that one dictum by Jeans³ seems to be fully justified: 'Space and time themselves must at least be of the same general nature'.

(ii) Dimensionality of Time

'There is no difference between Time and Space except that our consciousness moves along it'.

H. Wells, *Time Machine*

There is but one number of time dimension. This is a general opinion which is adopted as readily as the number '3' for space dimensions. Yet, in distinction to numerous investigations dealing with the space dimensional theory, there is no general dimensional time theory. Its one-dimensionality is either assumed as a self-evident fact or is based on one of the following considerations.

Reichenbach⁴ connects time flow with causation chains in the world, Eddington⁵ and Schroedinger⁶ with entropy increase, although with a difference that the former considers it as universal and unidirectional, whereas the latter considers it as local and denies the existence of Eddington's 'arrow of time'. On the main point, they agree that time has just one dimension.

Although this conception is the most widely represented, it is not the only one possible. Several authors have written concerning two-

¹ K. Schwarzschild, *Sitzber. Preuss. Akad. Wiss.*, 1916, I, 189

² P. Jordan, *Schwerkraft und Weltall*, Brunswick, 1952, p. 85

³ J. Jeans, *Physics and Philosophy*, Cambridge, 1948, p. 68

⁴ H. Reichenbach, *The Philosophical Significance of the Theory of Relativity*; Albert Einstein: *Philosopher-Scientist*, New York, 1951, p. 289

⁵ A. Eddington, *The Philosophy of Physical Science*, Cambridge, 1949, p. 198

⁶ E. Schroedinger, *Nature*, 1944, 153, 704

and three-dimensional time.¹ The latest treatment of time as a two-dimensional entity was published by Dobbs² in the pages of this *Journal*. Dobbs introduced a distinction between the 'transition time' and 'phase time' and treated the main points of his theory, in particular, the 'specious present' problem from the psychological viewpoint.

Adherents of the conventional one-dimensional theory of time frequently employ psychological and physiological criteria, as did Eddington³ in connecting our perception of the flight of time with molecular processes in the brain.

This approach should not become a starting point in any physical theory of time, however suitable it may prove to be in psychology. The time of physics should be clearly and unequivocally distinguished from the perceptual time of psychology. Our primary concern here is the former.

What must be understood under the name of 'physical time'? In our opinion, it is a factual constituent of the world substratum, be it called 'aether' or otherwise, which is on the same footing as another constituent, space.

What is the dimensionality of this physical time?

Jeans⁴ holds it as one-dimensional, as physical events can be arranged in a linear sequence. There are, however, several fundamental phenomena not accounted for within the framework of present conceptions: existence of opposite electric charges, positive and negative; opposite interactions, attraction and repulsion, etc. Eddington's 'Fundamental Theory', which contains many deep thoughts, as justly pointed out by Whitrow, introduced the conception of opposite 'chiralities' having a direct bearing on electric charges. Yet, it leads to the notion of a two-dimensional physical time, although the author did not want to admit this.

This is true, of course, if time is considered as a continuous quantity, as in the conventional theory. So, time is characterised by Weyl⁵ as the most fundamental continuum, in which the Archimedean axiom

¹ A. Calinon, *Etude de cinématique à 2 et à 3 dimensions*, Paris, 1890; M. Boucher, *Essai sur l'Hyperspace, le temps, la matière et l'énergie*, Paris, 1927, 3rd edn., p. 62; G. Bragdon, *Fourdimensional vistas*, New York, 1930, 3rd edn., p. 76

² H. Dobbs, this *Journal*, 1952, 2, 122 and 177

³ A. Eddington, *Space, Time and Gravitation*, Cambridge, 1953, p. 13

⁴ J. Jeans, *Physics and Philosophy*, Cambridge, 1948, p. 57

⁵ H. Weyl, *Das Kontinuum*, Leipzig, 1918, p. 67

is unconditionally valid, and the 'Zeitpunkt' represents a basic conceptional category.

This time structure, however, was assumed on the postulational basis. As soon as we recognise that this is an axiom, which also has no *a priori* or experimental grounds, we can explore the possibility of discarding it and adopting its opposite, discrete structure. In this case, too, the usual meaning of dimensionality will be lost, and physical discontinuous time will be represented by a zero-dimensional complex of cells, which are physical time instants having some minimum extension or duration τ_0 .

If both space and time are zero-dimensional cell complexes, does this mean that they are dimensionally equivalent? So-called 'common sense' discards such a conclusion for a simple reason: there are many directions in space and only one in time. Our 'common sense' judges on the basis of our perceptions, and the perception of different dimensions rests upon our ability to differentiate between various directions. This ability is rooted in physiological and psychological properties of the human being. So, he is equipped with semicircular canals to discriminate between spatial directions. The rôle of these canals was very extensively investigated and summarised by Cyon¹ as follows: 'Die durch die Erregung der Bogengänge erzeugten Empfindungen sind Richtung—und Raumempfindungen. Diese Empfindungen dienen dem Menschen zur Bildung der Vorstellung von einem dreidimensionalen Raume, auf den er seinen Seh—und Tastraum projicirt'. A hypothetical human being, deprived of these internal organs and whose body was of a perfectly symmetric (e.g. globular) shape, could hardly reach the conception of a three-dimensional space.

Turning now to the perception of time, we can see that the human being is very poorly equipped in his time perception in comparison with that of space. Although many directions in time are theoretically possible, due to turnability of the time-axis with high-speed motion, there is no organ in the human body to discriminate between them. Even one direction in time, that is actually pursued, is not directly perceived, because one single point, the present instant, is not sufficient; it does not define a direction, for which at least two points are needed. Our immediate awareness in time is limited to the present instant only (remembrance of past instants and anticipation of future ones are not comparable with direct perception of spatial points

¹ E. v. Cyon, *Ohrlabrynth, Raumsinn und Orientierung*, Bonn, 1900, p. 275.

already passed and forthcoming, which can be immediately observed). There is no time perception organ in the human body. Some hypothetical being, provided with such an organ and possessing an ability to be directly aware of past or future instants, would devise quite a different notion of time, perhaps two- or multi-dimensional.

Thus, we ascribe different numbers of dimensions to space and time not because of an intrinsic difference between their dimensions, but because of specific peculiarities in our psychological perception of time, i.e. because 'our consciousness moves along it'.

(ii) *Discontinuity of Space and Time*

'We still use it (concept of the continuum) for space and time; . . . but it may very well turn out to be out of place for physical space and physical time'.

E. Schroedinger, *Nature and the Greeks*

We discarded the postulate of continuity and Archimedean postulate and adopted tentatively the opposite; the hypothesis of discrete structure of space and time; that is, the existence of the least length l_0 and shortest duration τ_0 . Does this hypothesis have any grounds? First of all, we have to mention that this notion is not new at all. The first consequent proponents of discontinuous structure of both space and time, although from a purely philosophico-theological viewpoint, were medieval Arabian interpreters of the Koran, Mutakallimun.¹ A hundred years ago the discrete structure of space was surmised by Riemann² as one of the possible grounds for its metric properties.

In recent times, strong arguments in its favour were provided by some inconsistencies in the modern physical theory. The most significant of them is infinite self-energy of the point-electron, which indicates the meaningless application of zero distances to the real physical world. Although many ingenious attempts have been made to avoid this and other similar infinities and divergences, this struggle against infinities very much resembles the famous Don Quixotian struggle against windmills, for they were first imagined as real enemies and then attacked with a zeal deserving more constructive application.

As early as in 1930, Ambarzumian³ proposed to eliminate the difficulty of electron infinite self-energy in a reasonable way, by

¹ K. Lasswitz, *Geschichte der Atomistik*, Hamburg, 1890, I, p. 134

² B. Riemann's *gesammelte mathematische Werke*, ed. by Weber, Leipzig, 1876, p. 254

³ V. Ambarzumian, D. Iwanenko, *Zeit. f. Phys.*, 1930, 64, 563

introducing a spatial lattice structure. Schild¹ extended this idea to spatio-temporal lattice with a fundamental length of the order of 10^{-13} cm. and proved that the model of discrete space-time 'admits a surprisingly large number of Lorentz transformations'. March, Flint² and others also developed theories of discrete space-time, which proved to be self-consistent and free of any logical or physical contradictions. This circumstance alone indicates that the postulate of space-time continuity cannot be proven by 'reductio ad absurdum', by adopting its opposite; i.e. discontinuous structure.

If discrete structure of space is adopted as a working hypothesis, time must be assumed as discontinuous, too, in view of similarities pointed out above and their interrelation. Indeed, with space and time being inseparable and if space is discontinuous, time cannot be continuous, for otherwise there is no way of ordering continuous time instants to discrete space points; in this case, infinity of instants in the time continuum would be left without their spatial counterparts.

Another argument was cited in the above mentioned Ambarzumian article: the existence of the least length l_0 implies the existence of the least wavelength, and this leads to the maximum possible frequency or the least time interval $\tau_0 = \frac{l_0}{C}$. Thus it is a necessary logical consequence, that discreteness of time must accompany discreteness of space.

Are there any indications in Nature itself that these least quantities of space and time, their so-to-speak quanta, have actual physical reality? It is perhaps not without interest to cite a few figures indicating the possibility that this is the case.

Let us consider a physical point-instant, or quantum of space-time, as an irreducible spatio-temporal volume defined by the expression:

$$V_{tr} = \frac{4}{3}\pi l_0^3 \tau_0$$

Assuming l_0 to be of the order of magnitude of a proton radius, i.e. $\sim 1.5 \times 10^{-13}$ cm., and the value of $\tau_0 = \frac{l_0}{C} = 0.5 \times 10^{-23}$ sec., we obtain the value of $V_{tr} = 7.1 \times 10^{-63}$ cm.³ sec. If we imagine this spatio-temporal quantum to be filled with matter of nuclear density, $\sigma \cong 10^{14}$ g/cm.³, its energy content will be

$$V_{tr} \sigma C^2 \cong 6.4 \times 10^{-27} \text{ erg-sec.},$$

¹ A. Schild, *Canad. Journ. of Math.*, 1949, 1, 29

² H. Flint, *Phys. Rev.*, 1948, 74, 209

which is of the same order of magnitude as Planck's quantum $h = 6.624 \times 10^{-27}$ erg-sec. An idea suggests itself that the elementary quantum of action has something to do with discrete structure of the world, and with the elementary and irreducible quantum of space-time; that is, with the natural standard of length in the pure form, as was suggested by Whyte.¹

Let us now turn to the outer limits of space and time. Weyl² once put forward an idea that the non-Euclidicity of world space may mean that it has not only outer, but also inner limits. We shall now reverse his argument with respect to time. The limitation of time in the small to some indivisible quanta τ_0 may mean the existence of outer limits; that is, the existence of the longest time interval possible, instead of infinite time, which would be just another logical consequence of renouncing the Archimedean axiom.

We have thus come, in another way, to the same conception of finite time, which this author suggested in this *Journal*³ some time ago.

Now let us determine, in what relation the world of finite extension in space and time stands to its discrete structure. Assuming the value of space radius R to be equal to 3.4×10^9 light years, and the radius of time curvature $\rho = 3.4 \times 10^9$ years, as determined by this author⁴ the spatial volume of the closed Riemannian world will be

$$V = 2\pi^2 R^3 = 8 \times 10^{83} \text{ cm.}^3,$$

the duration of the finite circular time will be

$$T = 2\pi\rho = 6 \times 10^{17} \text{ sec.},$$

and the spatio-temporal volume will amount to $\sim 10^{101} \text{ cm.}^3 \text{ sec.}$ With the value of one spatio-temporal quantum of the order of $10^{-61} \text{ cm.}^3 \text{ sec.}$, as estimated above, the number of these units in the world will be $\sim 10^{162}$.

The so-called 'cosmic number' N , being the number of elementary particles in the cosmos, was estimated by Eddington⁵ to be of the order of 10^{80} . There is perhaps some relation between these two numbers, the latter being possibly a square root of the former, thereby connecting the atomistic structure of matter with discrete, quantised structure of space-time. Weyl⁶ once wrote in another connection:

¹ L. L. Whyte, this *Journal*, 1951, I, 303

² H. Weyl, *Raum. Zeit. Material*, Berlin, 1921, 4th edn., p. 238

³ 1954, 5, 237

⁴ *Ibid.*, p. 245

⁵ A. Eddington, *The Philosophy of Physical Science*, Cambridge, 1949, p. 177

⁶ H. Weyl, *Naturwiss.*, 1934, 22, 145

'Das Auftreten der Wurzel ist nicht unverstaendlich, . . . In den Statistischen Gesetzen tritt ja haeufig die Quadratwurzel aus der grossen Zahl der Einzelfaelle auf.' This consideration is applicable also to the present case.

Of course, only orders of magnitude can be compared; the exact coincidence of values in both cases above are not to be expected, in view of our still imperfect knowledge of the numerical values of lengths involved, and even more, our ignorance of the geometric structure of the world as a whole and its elementary quanta of space-time.

5 Conclusions

'solche Untersuchungen, welche, wie die hier gefuehrte, von allgemeinen Begriffen ausgehen, koennen nur dazu dienen, dass diese Arbeit nicht durch die Beschraenktheit der Begriffe gehindert und der Fortschritt im Erkennen des Zusammenhangs der Dinge nicht durch ueberlieferte Vorurtheile gehemmt wird.'

B. Riemann, *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen*

The problem of dimensionality of our physical space and time is closely connected with and depends on the solution of the other fundamental problem: continuity or discontinuity of their structure. Both of them are topological problems, which deserve more attention than was paid them thus far.

The numbers '3' and '1' are conventionally ascribed to dimensions of space and time respectively, when continuous structure of both was presumed as an axiom. The choice of these particular numbers was determined by the physiological and psychological properties of the human body (asymmetry, existence of space organs, non-existence of time organs, etc.). As soon as the postulate of continuity is dropped and physical spatio-temporal point-instants are assumed to possess some finite extension, dimensionality of both space and time loses its conventional meaning and absolute character.

If the Archimedean axiom is not applicable to our physical world, and its spatial and temporal dimensions are finite, the world's content is characterised not by its volume or any other dimensional quantity, but by the number of its quanta or spatio-temporal physical points; i.e. by a dimensionless quantity or a pure number, in accordance with the view advocated as early as twenty-five centuries ago by the Pythagorean school.

PHYSICAL SPACE AND TIME

Hereby it is not denied, that for the majority of today's problems, it is convenient to ascribe three dimensions to space, as admitted by Poincaré, and one dimension to time. However, the categorical statement that 'our physical world has three spatial and one temporal dimensions' should be replaced by a more modest one: 'At the present stage of human knowledge, most physical phenomena can be described as if happening in a three-dimensional space and one-dimensional time, assuming conventionally they both are continuous, or in a world, the line-element of which is expressed by a differential equation of signature $(+++ -)$ '.

Let us draw an analogy with Plato's prisoners chained in a cave and capable of seeing only shadows of the real objects outside of the cave reflected on its back wall. Their viewpoint would probably be characterised as follows: 'our physical world consists of two-dimensional colourless beings moving silently before our eyes'. Let us imagine now that some ingenious inventor in this cave devised a device, which renders shadows stereoscopic, another devised a method of reflecting colours, etc. The new picture of the physical world composed by the cave inhabitants would radically differ from the initial one.

Is it impossible, that analogous stereoscopic or other, figuratively speaking, improvements may happen in our description of physical reality?

Is it absolutely improbable, that a second, conventionally speaking, time dimension in one form or another may appear in the expression of the line-element?

Can an idea that the expression of line-element in differential form may be replaced by a difference equation, in which finite differences reflecting quantised properties of the world substratum will take over the former rôle of differentials, be discarded as unthinkable?

These and perhaps other developments cannot be denied *a priori*. Therefore, the statement regarding '3 + 1' dimensionality of our physical world must not be considered as an unquestionable and ultimate truth, excluding all alternatives, if 'the progress in understanding the connection of things,—as Riemann declared once—should not be hampered by traditional prejudices'.

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TWO EVOLUTIONARY THEORIES (I) *

MARJORIE GRENE

1 *The Two Theories*

1959 will find the majority of Western biologists assenting with renewed enthusiasm to the basic principles enunciated a century ago in *The Origin of Species*. But there continues to be an heretical minority, and, moreover, the heretics speak with authority and vigour. When, in turn, the orthodox pause to answer the dissenters' arguments, they offer an illuminating paradigm of scientific controversy: illuminating both for a study of the nature of scientific disputes in general and for the epistemological problems inherent in evolutionary controversy in particular.

I propose to limit myself here to the analysis of one such controversy: the disagreement between the theories of evolution stated by two palaeontologists, G. G. Simpson in *Major Features of Evolution* (1953) and O. H. Schindewolf in *Grundfragen der Paläontologie* (1950).¹

Professor Simpson is the principal American spokesman of neo-Darwinism, or, as he now prefers to call it, the 'synthetic' theory, and his *Major Features* is certainly one of the most carefully elaborated statements of this position.² The very fact that he speaks as a palaeontologist lends weight to that statement. Darwin himself had trouble with palaeontology;³ and there have been palaeontologists ever since who have failed to adhere to the Darwinian position even

* Received 13.i.1958

¹ I have drawn also to some extent on other writings of the same authors, notably G. G. Simpson's *Meaning of Evolution* (1949) and two addresses by Schindewolf: 'Evolution vom Standpunkt eines Paläontologen', *Schweiz. Pal. Gesell.*, 1952, 374-386, and 'Evolution im Lichte der Paläontologie', *Comptes Rendus, Congrès Geol. Internat.*, 1954, 93-107.

² It seems to me fair to call Simpson's theory neo-Darwinism if one recognises the differences introduced by the Mendelian basis of the newer type of theory. In essentials it is still Darwinism.

³ *Origin of Species*, Chapters X, and 6th edition, Chapter VII, answer to St George Mivart on abrupt transitions of form.

TWO EVOLUTIONARY THEORIES

when it had become an orthodoxy. Darwinism—and in this neo-Darwinism is no different—presents a picture of life streaming endlessly forward, confined in this or that channel only by the combined effect of environmental change and natural selection, which selects the useful by breeding out the less useful. This picture, however, has seemed to some palaeontologists a distortion of the fossil record, and particularly in two ways.¹ First, the record itself is full of gaps; forms appear and disappear suddenly. There are a few famous transitional forms, like *Archaeopteryx*, but not nearly all there should be if evolution really happened as gradually as the Darwinians think. Secondly, such continuities as the fossil record does exhibit often seem to be unrelated to utility (or in modern language to adaptation) and hence to lie beyond the control of natural selection. The stock examples of this are found in the sabretooth tiger and the Irish elk. Respectively they have teeth or antlers that go on and on getting bigger and bigger even after they become a nuisance, and this was said to illustrate the phenomenon of 'orthogenesis'. This concept is still used by some scientists (including Schindewolf), but the problem it raises really forms part of a larger question: are there important phenomena in evolution which appear to be non-adaptive, and which, therefore, cannot be reasonably interpreted under the aegis of selection? Up to recent years, at least, many palaeontologists thought there were such phenomena: they thought, in other words, that the concepts of adaptation and selection were not adequate for the interpretation of their subject matter.

Against these opinions we have Simpson, himself a most distinguished palaeontologist, the foremost living authority on equid history and a pioneer in work on rates of evolution, lending the weight of his authority to the neo-Darwinian view. He sees evolution as a continuous series of minute changes in innumerable directions, in which all alterations of any significance, larger as well as smaller, quicker as well as slower, are determined by the great co-operating 'pressures' of mutation, geographical isolation, and selection, with adaptation as the universal effect, and criterion, of systematic change. The basic concept, ultimately, is variation in the occurrence of genes; out of such variations all the systematic relations of living things have been gradually evolved.

¹ See for example H. L. Hawkins, *Rep. Brit. Ass.* (Presidential Address to the Geological Section), 1936, pp. 57 *et seq.*; H. F. Osborn, *U.S. Geol. Surv. Mon.*, 1929, 55; *Am. Nat.*, 1934, 68, 193-235; L. F. Spath, *Biol. Rev.*, 1933, 81, 418-462.

There are a number of prominent biologists who are dissatisfied with this view;¹ Simpson, in *Major Features*, takes special pains to reply to the critique of one of them: the German palaeontologist Schindewolf; and it is with their disagreement that I propose to deal.

Schindewolf finds the fossil record full of striking discontinuities, even where there can be no serious question of gaps in the record; new structures just suddenly turn up. Such new structures, moreover—for instance, the limbs of tetrapods—were not, he believes, in themselves adaptive. Land dwellers *might* have started with two legs, or half a dozen, or, octopus-like, with a ring of tentacles. How can we say that four is the best-adapted number, or that the pentadactyl ground-plan is the best-adapted pattern for such limbs in all the great variety of circumstances in which they have come to be used? This happens to be the characteristic tetrapod 'Bauplan', but there is no conceivable reason of utility why this number and no other, this blueprint and no other² should be found. Once present, of course, these four pentadactyl limbs did indeed develop in specialised, adaptive ways: for running, digging, jumping, etc. Their common structure, however, was not in itself, in Schindewolf's view, an adaptation, it was simply a new type of organisation, four-footedness, capable of numerous and varied adaptations.

Furthermore, Schindewolf agrees with the older palaeontologists that within each type, once it has appeared, there is a progressive, orthogenetic development. In fact, there is a rhythm analogous to that of birth, maturation, and senescence: the sudden appearance of a new type, its orthogenetic advance, and finally a stage of the breaking up of types which usually leads to extinction.³ The choice between a paraxonic and mesaxonic limb, for example, appears to be, from the point of view of adaptation, a matter of indifference. For speed of locomotion, either will do. Once the choice was made, however, there was a progressive, orthogenetic trend within each form, to the

¹ I may mention, in this country, four scientists who have expressed themselves as, in various ways, dissatisfied with the new synthesis (although I do not suggest that they would be in agreement, either with what I am saying here, or with one another): Professor H. Graham Cannon of Manchester, Professor Paul G. 'Espinasse of Hull, Professor Ronald Good of Hull, and Professor C. H. Waddington of the Institute of Animal Genetics in Edinburgh.

² The same point is made in a different context in Professor Cannon's recent book, *The Evolution of Living Things*, Manchester, 1958.

³ Schindewolf calls these stages *typogenesis*, *typostasis*, and *typolysis*, and his theory as a whole *typostrophism*.

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horse on the one hand, and the camel on the other. The major type—i.e. the structural plan of artiodactyl as against perissodactyl limb—had to come first, and within this the development of specialised forms occurs—e.g. cows, pigs, and camels as against horses, rhinos, and elephants.

Notice, moreover, that in each case the more general category appears first, and specialisation is specialisation *within* it. Whereas for Simpson it is a question of particular small changes adding up to big ones, for Schindewolf the big changes in structure come first, the new general structures breaking down thereafter into more special adaptations. This is related, as we shall see, to the fact that Schindewolf looks at structures first, putting morphology ahead of phylogeny, while Simpson, like all Darwinians, wants to make phylogeny—the history of life—the one foundation on which all the rest of biology is built.

2 *Parallelism of the Two Theories*

We have here, then, a nicely balanced pair of theories: one stressing continuity and the adaptive character of all evolutionary change, the other stressing discontinuity (novelty of forms) and the non-adaptive character of major changes. The two writers disagree seldom, if at all, about the 'facts', yet each rejects emphatically the other's inferences from the facts and is at pains to refute them. Consider briefly how the two views balance one another in this respect.

(a) Sometimes the very same matter of fact seems to the two of them to provide evidence for diametrically opposite conclusions. For instance, the fact that some very old genera, like *Lingula*, still survive, is proof, for Simpson, that evolution does not exhibit the general pattern of racial life and death: it cannot be said to go on in a cyclical fashion if some old survivors are left happily in their ancient and well-established niches.¹ The same sort of fact is proof, for Schindewolf, that selection is by no means all-powerful, since if it were it would wipe out all simpler forms in favour of the more advanced, i.e. more efficient.²

(b) Each one accuses the other of committing the same or similar fallacies. About the question whether higher categories—orders, classes, phyla—originate before or after species, each one accuses the

¹ *Meaning of Evolution*, p. 192; cf. *Major Features of Evolution*, p. 233

² *Grundfragen der Paläontologie*, p. 404

other, in nearly the same words, of arguing retrospectively, and reading into the record what he himself wants to put there.¹ Did the class of birds originate, in one step, with *Archaeopteryx*, before the innumerable species and sub-species and varieties of birds had developed? Simpson says *Archaeopteryx* was a species like any other, originating by normal speciation from other reptilian species; only when we look back over the whole vista of evolution do we say, this particular species was the first of what has turned out to be a new class.² Schindewolf says, *Archaeopteryx* is a bird: i.e. an animal that flies by means of feathers. There were never any before and there have been a great many since—but the transition, from class Reptilia to class Aves, happened then and there.³ The principle, flight by means of feathers, was genuinely new. Only the neo-Darwinians, late in time, come and conjure away its essential novelty.

Or, again, each accuses the other of unnecessary and mystifying assumptions. Simpson considers that orthogenesis smacks of 'purposiveness'⁴—the worst possible name-calling in evolutionary controversy; and the division of the history of a type into stages analogous to birth, maturity, and old age, he considers viciously anthropomorphic and unjustified by the facts.⁵ Schindewolf, on the other hand, insists that while his use of orthogenesis and of the type concept is purely descriptive,⁶ nothing could be more mystifying than the prophetic vision of selection which the neo-Darwinians implicitly assume: the idea that selection selects what *will* be advantageous in another ten million years, and thus foresees advantages that will accrue to the remote descendants of the forms selected.⁷

True, a neo-Darwinian, accused thus baldly, would simply deny that he makes any such preposterous assumption. Selection, he says, selects what is useful in *each* generation, otherwise it would not be selected. There is no 'foresight' involved, but only the automatic and immediate control of variation through utility. Yet there is nevertheless some plausibility in Schindewolf's criticism; for there are many cases in which Darwinians are forced to relax this basic principle, and to loosen the connection between adaptation and selection. It

¹ Cf. *ibid.*, p. 273, and *Major Features of Evolution*, p. 350

² *Major Features of Evolution*, 342, 370; cf. pp. 347, 350

³ *Grundfragen der Paläontologie*, p. 126; cf. pp. 201, 277

⁴ *Major Features of Evolution*, p. 268

⁵ *Ibid.*, p. 233

⁶ *Grundfragen der Paläontologie*, pp. 430-431

⁷ *Ibid.*, pp. 413, 430-431

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seems highly unlikely, for example, that the very minutest beginnings of ultimately co-adaptive structures, like the appendages of crustaceans, should have been of immediate and constant advantage to their bearers.¹ What does neo-Darwinism make of such cases? Sir Julian Huxley tells us that *very slight selective values can now be seen to be effective, because evolutionary periods are so long.*² But can an incomprehensible happening become comprehensible by lasting a long time? Either one is simply describing the direction of a trend that has already happened, and 'selection' is just shorthand for such a description, rather than an explanatory concept; or 'selection' stands for some mysterious force which controlled the whole process, in Dobzhansky's phrase (to quote another eminent neo-Darwinian), *sub specie aeternitatis*.³ This sounds suspiciously like Schindewolf's foresight of selection, as does Simpson's statement: 'it is certain that if we can see any advantage whatever in a small variation (and sometimes even if we cannot), selection sees more'.⁴

To return to Schindewolf: as against the highly abstract and hypothetical Darwinian theory, he makes the assumption: *that life can originate novelty*.⁵ This is, he says, a preferable assumption because it is simpler. He does not pretend to 'explain' this proposition, and in that sense it may be 'mysterious', but no more mysterious, he says, than physical concepts like 'force' or 'gravitation' which everyone is prepared to accept as 'explanatory'—and not nearly so mysterious, he believes, as the whole nexus of assumptions implicit in the concept of 'natural selection' as the neo-Darwinians use it.⁶

(c) Finally, and most neatly characteristic of this type of situation, the two scientists argue in exactly opposite directions, each taking as his premiss the negation of the other's conclusion. Simpson, wedding palaeontology to the statistical methods of population genetics, sees a gradual change in populations such that the sharp divisions of traditional morphology become false.⁷ Schindewolf, basing his theory on the logical priority of morphology, concludes that the gradualist,

¹ A. Vandel, 'L'Évolution Considérée comme Phénomène de Développement', *Bull. Soc. Zool. de France*, 1954, 79, 341-356. Cf. L. Cuénot, *L'Évolution Biologique*, 1951; 'L'Anti-hasard', *Rev. Scient.*, 1944, 82, 339-346

² In Huxley, Hardy, Ford, *Evolution as a Process*, London, 1954, p. 3

³ T. Dobzhansky, *Genetics and the Origin of Species*, 3rd. edn., New York, 1951, p. 75

⁴ *Major Features of Evolution*, p. 271

⁵ *Grundfragen der Paläontologie*, pp. 425-429

⁶ *Ibid.*, p. 431

⁷ *Major Features of Evolution*, pp. 377-378

statistical picture of neo-Darwinism is false.¹ To put it very schematically; Simpson argues: the neo-Darwinian theory is true; morphology implies that neo-Darwinism is not true; therefore morphology is wrong. Schindewolf argues: morphology must first be accepted as true; morphology implies that the neo-Darwinian theory is wrong; therefore the neo-Darwinian theory is mistaken. Or to put the matter another way, they agree on their major premiss: *traditional morphology and neo-Darwinism are incompatible*. One says: Darwinism; therefore not morphology; the other says: morphology, therefore not Darwinism.

In each book this argument—that is, in effect, the argument on the relative priority of morphology and phylogeny—comes late; but in each book it is a most decisive argument, which has been in fact moulding the shape each theory has taken all along. Why is it so decisive? In neither case has a crucial experiment falsified the other theory. Nor is it merely that the two writers start from different premisses. But these different premisses, leading each to a conclusion directly contradicting the alternative premiss, are symptoms of a more pervasive difference, a difference in ways of thinking. To put it very simply, the two writers look at their material differently. It is this difference in 'looking' that I want to analyse and in part to evaluate.

3 *Planes of Disagreement*

When we consider the two theories a little more closely, we find that the differences between them are themselves of different kinds. Perhaps we may say that our two scientists disagree on different *levels* or *planes* of thought. There is, first, the *verbal* plane. They differ at many points simply in the formulation of observed phenomena or of their generalisations from them. Secondly, there is the *visual* plane. They use different sorts of visual imagery to support their theories; the models on which they rely are different in kind. Third, the plane of *attention*. They pay heed to different aspects of the phenomena. Different *kinds* of question interest them. Finally, and most fundamentally, we come to the *conceptual* plane. Their 'outlook', the interpretive framework *out of which* they are looking at their material, is different.² Here, and indeed on our second and third levels, we will

¹ *Grundfragen der Paläontologie*, pp. 300, 455 *et seq.*, esp. p. 475

² In terms of Professor Polanyi's philosophy of personal knowledge, the two are thinking on opposite sides of a *logical gap*; see M. Polanyi, *Personal Knowledge*, London, 1958, pp. 150-160. In terms of Professor Hodges' Riddell Lectures (*Languages, Standpoints and Attitudes*, Oxford, 1953), they differ *existentially*.

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have moved beyond the range of differences specifiable in terms of vocabulary and syntax, to the more pervasive *tacit* component of theoretical knowledge.¹

4 *The Verbal Plane*

Consider, then, first, the verbal differences between the two theories—those differences which *are* specifiable in terms of vocabulary and syntax. The interesting thing about these differences is that when one looks at them hard, they disappear. It looks as if a great many statements in the two theories could be translated without loss of meaning one into the other. This does not mean that their disagreement is 'merely verbal'. On the contrary, if their statements were all translated into a common terminology, they would agree verbally but would still disagree.

Let me mention briefly two examples of such verbal disputes. Simpson objects to 'orthogenesis' but speaks of 'essentially rectilinear evolution',² which is all that Schindewolf's 'orthogenesis' is shorthand for. Schindewolf insists that 'adaptation and genuine evolution are two quite different things', yet in speaking of the origin of mammals he says this was a 'general improvement' as distinct from the adaptations of particular groups of mammals to particular environments—as the limbs of bats, whales, etc.³ But Simpson also distinguishes between adaptations of a general and variable kind, and more limited and specialised adaptations—like the rodent's tooth.⁴ Why couldn't Schindewolf speak of 'general adaptations' instead of 'general improvement'? But he would be very angry at this suggestion, as Simpson would be if told that he might as well say 'orthogenesis' instead of 'essentially rectilinear evolution'.⁵ Obviously the verbal disagreement, which, in these and a number of other cases, could be easily eliminated, is relatively unimportant. It is the difference that would remain in spite of verbal agreement that matters.

¹ See Polanyi, *op. cit.* Part II, 'The Tacit Component'.

² *Major Features of Evolution*, p. 259

³ 'Evol. vom Standpunkt eines Paläontologen', p. 382

⁴ *Major Features of Evolution*, p. 346

⁵ See Simpson's note, *ibid.* p. 259: 'This means the same things *descriptively* as Schindewolf . . . means when he says that orthogenesis is characteristic of the typosstatic phase of evolution, although I reject the theoretical implications of his statement as decisively as he rejects mine.'

5 *The Visual Plane*

Let us try, then, to look behind the verbal level of the dispute and consider the differences that underlie it. The two scientists, we said, *look* at the facts differently. They differ in the visual imagery by which their theories are supported. In Simpson's case the visualisation is at a relatively abstract level. Sometimes he uses graphs illustrating

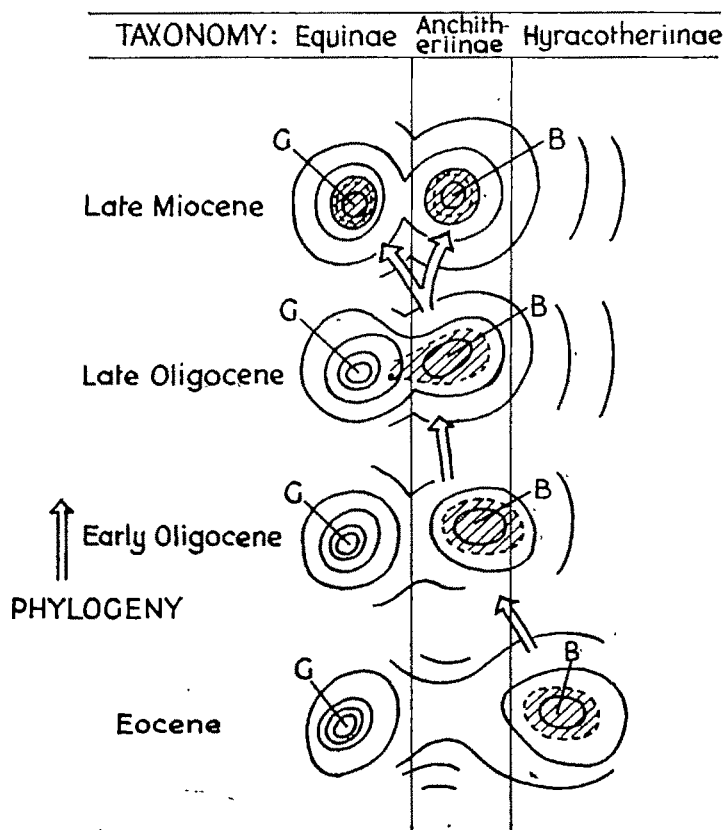


FIG. 1.

complex statistical relationships, sometimes models in the sense of cognitive maps. In Schindewolf's case it is usually, more directly, the actual shape of fossils that we are asked to envisage: these shapes and differences in shape illustrate the relationships characteristic for evolution.

Consider an example from each. Simpson (Fig. 1) summarises the development of browsing and grazing types in equid phylogeny in

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terms of the 'selection landscape' technique introduced by Sewall Wright.¹ Selection landscapes are constructed by applying abstract topological techniques to a wide range of data already subject to elaborate statistical manipulations. The result is a series of 'peaks' and 'valleys', each peak representing an adaptive optimum for the characters and groups in question.² In the case of horse evolution, we have two adaptive peaks, a browsing peak (B) and a grazing peak (G), with periods of shifting between them, and finally (not shown on this diagram) the disappearance of the browsing peak.

Compare with this a diagram of Schindewolf's (Fig. 2)³, showing the development of one type of modern coral (f-h) from an early ontogenetic stage (b) of an ancient coral (a-e). It seems unlikely, if we look at (e) and (h), that a transition between them could have occurred by small cumulative steps. We could imagine, however, that in a young Pterocoral, the third pair of protosepta (c III) failed to develop, and instead (as is the case with modern Heterocorals), the second (f II) and then the first pair (g I) divided laterally, producing the four divisions (and other novelties in the arrangement of the subdivisions) typical of the new kind of animal. This is something one sees intuitively, by looking at the actual shapes of fossil and recent corals at early and later stages of growth.

6 The Plane of Attention

The difference in visualisation is symptomatic, further, of a difference of attention. The two are looking at different aspects of their common subject matter. What is central for one is peripheral for the other, and vice versa. Simpson is applying statistical methods in palaeontology, in close reliance on the theories of population genetics, which are also statistical. He is dealing, not with individuals, which do not evolve, but with populations, which *do*. Schindewolf is looking rather at individuals of different shapes and sizes, and the problem is to relate them phylogenetically; this can best be done in reliance on analogies drawn from embryology. From these different perspectives the same 'facts' look different. For example, Schindewolf

¹ Based on *Major Features of Evolution*, p. 157 (Fig. 17). (By permission of the author and Columbia University Press.)

² Selection is represented as positive (uphill) or negative (downhill), its intensity being proportional to the gradient. See *ibid.* pp. 155-157

³ See p. 20. Based on *Grundfragen der Paläontologie*, p. 214 (Fig. 213).

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uses the development of artiodactyl and perissodactyl limbs evidence that major steps in evolution occur discontinuously—and that therefore his theory rather than the neo-Darwinian is correct.¹ Simpson cites the development of the artiodactyl limb as a case of what he calls quantum evolution, that is, evolution by an all-or-none

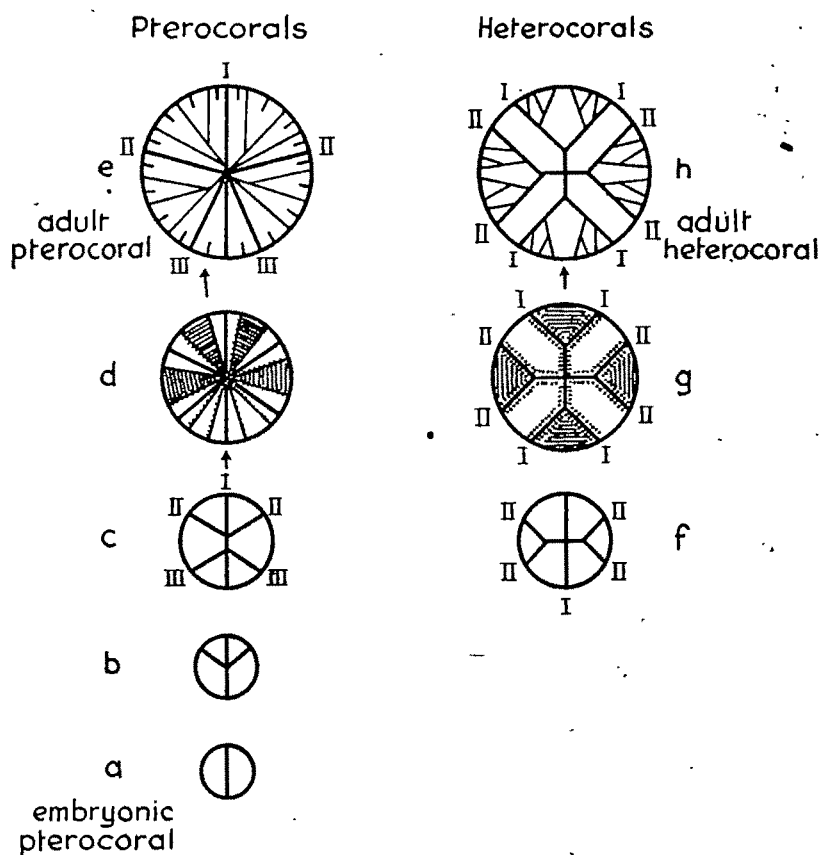


FIG. 2.

transition.² Here the two are even in verbal agreement, for Schindewolf describes his transitions from type to type as 'quantenhaft'. Moreover, both agree that the transition in this case constitutes a sharp dividing line. Yet the different field of attention in each case puts such divergences into a very different context. Simpson is looking at gradual transitions in the genetical make-up of populations, while

¹ Based on *Grundfragen der Paläontologie*, pp. 280-281

² *Major Features of Evolution*, pp. 391-392

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Schindewolf is looking at changes in the structure of individuals, which often vary slightly though sharply, and between two such sharply divided varieties organisms fall easily into 'types'. Thus Simpson sees a sequence of particular changes building up to a generalised trend, Schindewolf a general pattern with lesser changes within it.

Let us look more closely at Simpson's account. Populations, from the point of view of population genetics, are aggregates, not even of individual organisms, but of gene-complexes, tending to vary this way or that. A population is conceived in terms of its 'gene pool', that is, the sum total of the genes of its members' germ cells. 'Genetical selection' is the concept used to describe the systematic direction of the variation of genes, as distinct from drift, a minor factor, chiefly in small populations, which consists in fluctuations that fail to establish well-defined trends. Concepts such as 'threshold' or 'break-through' serve to facilitate the understanding of relatively abrupt shifts in such gradually changing trends; but what is of primary interest is the gradual change in the overall distribution of genes in interbreeding populations. That there was a first *Eohippus* or *Mesohippus* or *Equus* is not nearly so interesting as the fact that there were gradually more.

Attention to overall population trends, moreover, is supported by attention to the shifting organism/environment relationships—strictly speaking, gene-pool/environment relationships—which underlie them. Individual wholes are not of interest in this vision of pattern growing out of flux. And of course, it is primarily the 'mechanism' of these innumerable small changes that population genetics has so successfully interpreted. Minute alterations in the material of heredity do tend to appear in experimental populations, and to perpetuate themselves. Such systematic change in hereditary material is called 'genetical selection', and selection is said to be the 'mechanism' by which such change happens. It is not, however, a 'mechanism' in terms of a cause-and-effect relationship, for the occurrences it correlates are random. Nor are internal laws of structure or development of interest to this kind of evolutionary thinking. Selection in the sense of population genetics is simply any systematic change, regardless of cause, in the make-up of the gene-pool of an actively interbreeding population. The occurrence of mutations being taken as random, selection is the settling of these random occurrences into statistically manageable averages. It is represented by the curve established by reading averages of gene frequently over a number of generations. Except in

direct relation to reproduction, therefore, selection, in the sense of genetical selection, has no *intrinsic* relation even to adaptation. Simpson himself makes this quite plain. That such systematic change produces adaptation can be stated only on empirical evidence, or as a consequence of relaxing the strict genetical definition.¹

Schindewolf, on the other hand, is looking at the difference, say, in the shells of cephalopods or the structure of corals. The interesting point is not that the number of ammonites or of heterocorals came gradually to a peak, but that there were first ones, abruptly different from anything appearing in lower layers of fossil deposits. These changes cannot be interpreted as crystallising out of fluid gene-pool/environment relationships, since they often appear when there is no evidence of significant environmental change. In fact, in Schindewolf's view, it is by the change in fossils that the succession of strata must be ordered, not vice versa. Moreover, the new types often show no perceptible difference in adaptive relations. In the history of corals, the change from six to four divisions is not a change in adaptation. It is just a change in plan. What we are looking at is a set of forms, macroscopic structures, *Gestalten*, which are in their nature discrete. The problem, for example, for the two types of coral, is to account for the continuity between them: and here the concept of early embryonic change (proterogenesis, or in English usage paedomorphosis) comes to our aid. Such change is also mentioned by Simpson, but concerns him little; for what interests him is the statistical trends in *populations*, not the life history of the individual embryo. Thus, though it is Simpson who seems to have moved beyond a succession of stable fossil forms to the genetic basis of evolution, it is Schindewolf who sees these stable forms as relics of living individuals and connects them, in imagination, through concrete, developmental stages rather than through abstract, statistical change. It is true that populations, not individuals, evolve; but populations are populations of individuals,

¹ Dobzhansky, op. cit. pp. 79-80, equates 'adaptive value' with 'differential reproduction' (i.e. with genetical selection), thus concealing from the start the fact that the two are not conceptually equivalent. In this connection it is interesting that genetical selection is so often called the only known 'mechanism' of evolution, when it is by definition not a mechanism at all, but a statistically established trend. (See e.g. *Major Features of Evolution*, pp. 144-146). Strictly speaking, the 'mechanism' resides in the mathematical skill of scientists like Fisher, Haldane, Wright and Simpson himself. It is only in the older and looser Darwinian sense that selection is genuinely a mechanism in nature. Dobzhansky's definition glosses over this difficulty; Simpson admits it at the start but tends to forget his admission.

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and something that happened in individual development must have underlain the abstract statistical relationships which population geneticists construct. It is this underlying *real* change that Schindewolf is trying to envisage. In other words, where Simpson interprets the palaeontological data in conjunction with the abstract generalisations of population genetics, Schindewolf relies on the embryologist's conception of individual development.¹

In short, looking at populations throughout evolutionary history, we see a series of ever-branching particular directions establishing relatively stable interplay of gene-complex/environment relations. Both these variables change continuously, and so does the result, statistically viewed, of the outcome of their interactions. Looking at individuals, we see a multiplicity of types, which, once there, adapt themselves to particular variations in environment, and of course die when they become mal-adapted; but the initial divergence in types is the fundamental, and irreducible, starting-point. For Simpson, evolutionary change consists in the 'break-through' of a population into a new 'zone'. This may occasionally happen as an all or none reaction, but not generally. The so-called explosive phase of evolution is usually just a speed-up of normal speciation 'due' to intensive selection pressure in a new 'zone'. For Schindewolf, evolutionary change occurs when a youthful animal takes a new turning on its ontogenetic course, and so, as Hardy puts it, 'escapes from specialization',² with the result that something new—a new ground plan—appears which is withdrawn momentarily from the confining influence of selection, and proliferates, out of sheer exuberance, in all sorts of directions.

7 *The Conceptual Plane*

Our two scientists, then, not only lean on different kinds of visual images as models, but look at different aspects of the phenomena. Why these differences? Why do they use such different visual aids? Why is their attention so differently directed? The answer can be

¹ In this connection see the important essays of A. Dalcq on ontomutations, 'L'Apport de l'embryologie causale au problème de l'évolution', *Port. Acta. Biol. Vol. Jub. Goldschmidt*, Coimbra, 1949, pp. 367-400, and 'Les Ontomutations a l'origine des mammifères', *Bull. de la Soc. Zool. de France*, 1954, 79, 240-255; also A. Vandel, *op. cit.*

² Sir Alistair Hardy in Huxley, Hardy, Ford, *op. cit.* pp. 122 et. seq.

given only out of a third sense of 'looking': what they are looking *for*, the general perspective out of which they look, is different. They get different answers from nature, in Kantian terms, because they are putting different questions: and they put different questions because they stand at different places in relation to their subject-matter: their 'outlook' is different. This may sound like playing with words: one can only see what one is looking at, and one only looks at what one is looking for. But consider our two cases in terms of this question: what are they looking *for*? What is their outlook, their way of seeing? That is really the most fundamental kind of difference, on which the differences in verbal usage, in visualisation, and in attention, all depend.

Clearly, as I said at the beginning, Darwinism (and following it, neo-Darwinism) takes its stand firmly on the ground of continuity.¹ It sees life, and ultimately matter, as continuous and therefore explicable, in the last analysis, in terms of a *single* set of principles. Schindewolf's sort of theory envisages a variety of types, a set of discrete wholes or patterns, in step-like arrangement. This simple alternative may seem to be contradicted by the statement of Dobzhansky (certainly among the most authoritative neo-Darwinians) that the most striking phenomenon calling for explanation by the biologist is *discontinuity*.² But actually Dobzhansky's statement confirms my interpretation: continuity is the fundamental framework within which neo-Darwinian biologists are thinking, and discontinuity is the surprise they must explain away. They have to show how out of continuity the actual existent discontinuity—the biological discreteness of species, genera, etc.—could have arisen. Schindewolf, starting with the discreteness of different morphological types, has to explain their continuity. For him the panorama of forms comes first, and since these do appear, in a series of strata, with increasingly complex novelties succeeding one another, it becomes necessary to explain how transitions could have happened, as Schindewolf certainly agrees they did, between such basically different forms. Hence, as we have seen, his interest in early embryological change, for in this kind of change one can envisage an abrupt alteration in structure which nevertheless takes account of the actual continuity in the successive generations of living things.

Their disagreement about the relative priority of morphology and phylogeny lies close to the heart of the matter. Schindewolf looks

¹ This is not, of course, a mathematical continuum, but a series of minute changes conceived as functions of continuously changing particulars.

² T. Dobzhansky, *op. cit.* pp. 3-4

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first at structure, and can't understand how anyone can look at phylogenetic relations without having first looked at structure. But the great vision of Darwinism is precisely the dissolution of stable forms into a continuum. What Darwin did was to unsettle the fixity of species, to make nature flow. If he threw Paley's watch into the middle of the ocean, life itself became that ocean; in Simpson's image of selection landscape: 'rising, falling, merging, separating, moving laterally, at times more like a choppy sea than like a static landscape'.¹ And once life is seen as flux, change in its 'forms' is reduced to nearly imperceptible transition, the gradual elongation of the elephant's trunk or the gradual disappearance of the equid's extra toes, and so on.

If, however, the stress on continuity is essential for neo-Darwinism, that conception alone is by no means definitive for the neo-Darwinian outlook. A Lamarckian theory would also stress continuity, through the gradual establishment of new techniques to meet new needs. A Lamarckian theory would also, of course, stress adaptation, which the Darwinian view, as against Schindewolf's, also emphasises. What differentiates the neo-Darwinian theory is the peculiar combination of three basic concepts: continuity, particulate inheritance (and mutation), and adaptation. What is this peculiar combination? How do these three ideas hang together in the neo-Darwinian conception of evolution?

First, let us examine the relationship between the particulate mechanisms of heredity and changes in heredity. This at first sight looks incompatible with the emphasis on continuity and gradualness of change and, of course, mutationism was for a while thought to contradict the basic Darwinian view. But as Fisher declared twenty-five years ago,² the knowledge that mutations are the bearers of evolutionary change in fact proves the triumphant support, the very keystone, of neo-Darwinism, and the want of this knowledge was precisely what made Darwin's own position sometimes inconsistent. How, conceptually, do continuity and the particulate character of hereditary material go together? In two ways. First, to think in terms of small building blocks, like genes, is to think in terms of parts, not wholes, or of populations of such parts, that is, of collections of particulars, which again are not wholes. Moreover, as we have seen, the populations of population genetics are not even collections of whole

¹ *Major Features of Evolution*, p. 157

² Sir Ronald Fisher, 'The Bearing of Genetics on Theories of Evolution', *Science Progress*, 1932, 27, 273-287

organisms, but collections of gene complexes, and a gene complex, again, is a sum of independent particulars. Even though its *effect* be conceived as integrated, in its constitution it is of a mosaic rather than a comprehensive kind. It is not an organic whole, but a juxtaposition of small discrete particulars. In short, both types of concept: continuous variation or change, and particulate inheritance with its minute though discrete alterations are, precisely, devices for *not* looking at wholes or Gestalten or structures: for reducing organic structure, on the one hand, to values of continuous functions, and on the other, to collections of minute particulars. Here, I believe, we really meet the ruling passion of Darwinism: in the determination not to look at structure. Structure must be explained *away*; it must be reduced to the conditions out of which it arose rather than acknowledged *as* structure in itself.

This first part of the answer is negative, and admittedly does not take us far, since particulate pattern and continuity are so obviously discordant conceptions. The second part, however, is positive: this is where population genetics steps in to reconcile the discrete and the continuous. It is a most powerful instrument for synthesis since, statistically handled, collections of genes, which are particulate, do nevertheless vary continuously. Combined with Mendelian statistics—which are themselves grounded in a particulate, non-structural view of the nature of organisms—the statistical methods of population genetics allow one, therefore, to view the fluctuations in Mendelian statistics (which appear as the changing average of the relative frequency of one gene over its alternative in interbreeding populations) as producing a continuous process of evolutionary change. Nor is that all. Statistical calculations of rates of evolution, the construction of adaptive peaks and valleys, etc., which are included in the apparatus of population genetics, allow one to construct a highly abstract quasi-structure which appears to introduce a satisfactory—because abstract—pattern into an otherwise unintelligible flux.

What about adaptation? Again, the central role assigned this concept does not follow from the concept of continuity in itself. Leibniz saw the world as a continuous array of degrees of perception, not of adaptations. In fact, it seems odd that the very theorists who most emphatically deny any shred of purposiveness to nature, should just as emphatically declare that all significant changes in nature are adaptive in character. But the stress on adaptation falls into place once one takes the refusal to look directly at structure as fundamental. It is the shifting organism/environment relation, not the form of the

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organism itself, that is the basic unit. Organisms, already dissolved, from the perspective of evolution, into gene complexes, are themselves constantly changing as an (equally changing) environment plays upon them. Thus what changes is itself a product of two variables: average gene frequency and environment. There are no constants which would have to be assessed as patterns or achievements in themselves, only what Simpson calls the 'splendid opportunism' of life.

This is indeed a brilliant perpetuation of Darwin's vision, and its persuasive force is compelling. Schindewolf's principles are simpler. He sees typical shapes, and he sees again and again what appear to be new shapes. Therefore he assumes that living things are able to originate novel types. Mutation, he agrees, must have been the mechanism by which they originated; but the adaptive control of mutation occurs only within, not between types. The basic pattern is of change from type to type, and always, as we have seen, with the more general appearing before its specialised subdivisions. We cannot, in fact, describe specialisation except *within* a more general type: there must be some kind to become specialised.¹ First we have types, then we acknowledge the origination of new types. Within these, there is an inner dynamic of fulfilment: orthogenesis, and at the same time radiation into specialised subtypes controlled by adaptation and selection—the last and least creative steps, leading each time to an evolutionary cul-de-sac, whether as with *Lingula* to arrested evolution or, as with the majority of species, to extinction.

(to be concluded)

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¹ Simpson's argument that the higher categories are adaptive does in fact generally involve reference to a group which separates out *within* a still wider group—as bats, or rodents, or carnivores versus ungulates, and so on. See Chapter XI, *passim*, esp. pp. 346 et seq.

NOTE AND COMMENT

A Note on 'Theories, Dictionaries, and Observation'

IN my article¹ I overlooked some special circumstances under which a formal theory which is translated by means of an arbitrary dictionary into phenomenal statements may be said to be testable. Suppose the dictionary consists of n entries translating expressions of the theory into concepts $\alpha_1, \dots, \alpha_n$, which may be regarded as being operationally defined in the phenomenal language. Then there may be $(n - 1)$ independent functional relations between the α 's, all of which are directly testable. It may happen that a formal theory is constructed after only some of these have been discovered; it is then possible to say that the deduction and translation of the finite number of such relations remaining enables the theory-plus-dictionary to be tested.² Such tests are distinguished from others by requiring no new entries in the dictionary, and I shall call them *formal tests*.

If the phenomenal language is defined in such a way that it does not contain any concepts which imply correlations or connections not assumed in non-scientific discourse, then formal tests are usually, but not always, trivial. Most *applications* of a theory will be formal tests, for example if operational definitions are given of the time and space coordinates of astronomical objects, then the prediction of the orbit of a comet or an earth satellite is a formal test of Newtonian gravitation, and this is more usually regarded as an application than a test. But Newton's comparison of the force required to keep the moon in its orbit with the gravitational force at the surface of the earth was not a formal test, because his *phenomenal* language did not include a concept of force referring indifferently to celestial and terrestrial dynamics. On the other hand, Hertz's experiments on the transmission of electromagnetic waves were formal tests of Maxwell's equations, since they required no new operational definitions in the phenomenal language, and as a matter of historical fact these tests were not trivial. If, however, the formalism of Maxwell's equations had been known before the properties of magnetic

¹ This *Journal*, 1958, 9, 12.

² This is implicit in Professor Braithwaite's account of the 'four-factor' theory (*Scientific Explanation*, p. 68 ff). I should not have ascribed to Professors Braithwaite and Woodger point (d) on p. 14 of my article, since their theoretical statements do not constitute a distinct language, but merely contain theoretical concepts which are eliminated in the deduction of phenomenal statements. But my argument still shows that these theoretical statements cannot, in general, be tested.

needles (as might very well have been the case if electric currents had been discovered before magnetism), a prediction derived from Maxwell's theory about the behaviour of natural magnets would not have been formal, for the necessary interpretation of the theory would have involved some electronic model of matter and new operational definitions enabling the theoretical term 'magnet' to be interpreted as descriptive of actual bodies having a certain chemical constitution. Other, non-fictional, examples of non-formal tests were given in my paper.

If, however, the language into which a theory is interpreted for test purposes is the entire language of classical physics, as is the case for quantum theory, then formal tests may be more interesting and important. The situation here is somewhat obscured by the fact that quantum-mechanical formalisms have evolved by close analogy with classical theory, and hence it appears artificial to consider them as different languages and to suggest that any 'dictionary' between them is required. When a new formalism is introduced in quantum theory the immediate problem is to find its 'physical interpretation', and by this is meant an interpretation in specifically quantum language which while not phenomenal nor even classical, yet shows by the model which it exhibits what classical test-statements will be relevant to it. The whole procedure of quantum theory, with its insistence on physical interpretations, is in fact a refutation of the formal view of theories. But whether any particular prediction, for example of the properties of a new particle, constitutes a formal test or not, will depend on whether the formalism from which it is derived is already wholly interpreted in classical terms or whether it contains some symbols whose classical interpretation is not known. The discovery of the positron, for example, was a non-formal test, since it involved the new hole-theory interpretation of a quantum-mechanical formalism which had been *formally refuted* by the non-appearance of particles of negative energy.

MARY B. HESSE

DISCUSSIONS

WHAT MAKES A SUBJECT SCIENTIFIC?

W. B. GALLIE's paper in this *Journal*¹ under the above title does not expose an ambiguity in the question itself, which may represent an inquiry as to whether there is anything about a subject ('whether in its nature, its treatment, or otherwise) that marks it decisively as scientific; or may ask simply what it is that makes a potentially scientific subject an actual scientific subject in the sense that it qualifies for inclusion in the conventionally recognised scientific tradition or discipline. The importance of drawing attention to the second and subsidiary question lies in the danger that the answer to it (which is easy to find and quite irrelevant to the substantial question) may be offered as a solution to the problem proposed. The paper referred to does not appear to have avoided this trap.

It will help to clear the ground if this secondary question is disposed of, and this is most readily done by taking a concrete example. Farming is a potentially scientific subject, but as studied by the average farmhand it hardly qualifies for recognition as a scientific subject in the secondary sense. To rank as agricultural science it must be studied systematically upon the basis of a sufficiently elaborated and ordered public record or tradition of information and opinion. Just how systematic the study must be, and how well-developed a record is required to effect the transition, is a matter, not for definition, but for agreement. A conventional standard must be established as a yard-stick and even then, there will be room for disagreement as to whether the subject as studied measures up sufficiently to qualify for recognition. It is plainly misconceived to attempt to frame a definition of a scientific subject that would exclude farming from agricultural science. Nevertheless, the demand for a standard of study on the basis of a reasonably established public record is apt to give the impression that the scientific character of a subject is in some way related to publicity, tradition, and history. It may also lead to the suggestion that such matters as communicability, verifiability of data, and a high degree of systemisation are *indicia* of science. The fallacy lies in the fact that all the requirements mooted along this line apply in equal force in every discipline. Thus, the legal discipline emerged with the reporting of case-law, the promulgation of statutes, and the issue of treatises designed to establish a systematic order out of the information so gathered. A subject can only be recognised as a legal subject, in the secondary sense, if it is systematically studied by reference to the tradition established by that means. The fact that a subject forms a well-recognised part of either the legal or scientific disciplines, and is rooted in the appropriate historical tradition, may be very cogent evidence that the subject is legal or scientific (as the case may be); but no light is thrown thereby upon why the discipline is legal or scientific, and we still do not know why the subject has been drawn into the one rather than into the other. It is difficult to resist the conclusion that the scientific character of a subject does not depend upon its forming a part of the scientific discipline but that it falls into that discipline because it has a certain character that we

¹ 1957, 8, 118 et seq.

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deem, for reasons we cannot identify, as scientific. The substantial question is thus left isolated, and a number of convenient argumentative bolt-holes have been closed.

This is not the place to review the *indicia* that have been proposed to distinguish scientific subjects or activity, nor is it necessary in view of the lucid and admirable paper that has prompted these comments. It may be permissible, however, for a lawyer to propose a series of interconnected definitions that result from an attempt to mark out the frontier between science and law in the no-man's-land of psychology. It is recognised that, in the form proposed, these definitions are too gross for acceptance by scientists as they stand, but reasons will be advanced for supposing that they may point in a direction worth further consideration.

1. *The province of science* is the whole area of human interest that can profitably be studied without the need to assume any factor of responsibility in the subject-matter of study to account for its condition or behaviour ;
2. *The scientific method* is any method that can profitably be employed without making the assumption of responsibility referred to ;
3. *A scientific subject* is one that (a) is amenable to study in accordance with the scientific method and (in the secondary sense) (b) is studied in accordance therewith sufficiently methodically by reference to an adequately developed public record of information and opinion ;
4. *Science* is the body of recorded information and opinion that represents at any given time the public tradition arrived at by the study of scientific subjects in accordance with the scientific method.

From the scientist's point of view, the keyword of this series of definitions, namely responsibility, is so dangerously loose and indeterminate that their value must, at first sight, appear dubious to say the least. Nor is it easy to find a satisfactory substitute. Were it not for the doubtful uses to which the term, subjectivity, is put, this might be preferred. But even if no satisfactory term is found, it is not difficult to appreciate the crucial factor which the scientist either does not find or, for the purposes of his study, declines to employ as a methodological instrument. Whatever it may be called, it represents a certain liability to criticism or approval. The scientist does not catch himself out saying to the subject-matter of study: 'Good effort ! One more trial and you will pull off what is expected.' The responsibility-factor attaches to himself and to his colleagues, and not in the least degree to the subject studied. This factor might, perhaps, be regarded as a capacity (however small) to select the response to experimental interference.

If the scientist is content, despite the obvious difficulties, to accept the definitions provisionally to see what clarifications, if any, they achieve, he may, even though still dissatisfied, deem it useful to explore the general direction in which they point, namely, the nature of the methodological *assumptions* employed by scientists.

The definitions plausibly suggest that there is a certain unique economy in working assumptions in scientific method that produces what we call 'science'. This enables us to see precisely why it was necessary for the animistic assumption to wither so that science could take root in its primitive forms; and it explains also, why scientists at a much later date were obliged to resist the introduction of God as a methodological instrument of explanation despite the blandishments and threats of theologians who were alarmed lest this scientific license should lead to the outright rejection of God as a factor to facilitate explanation in other fields of human interest. We can see more

clearly, too, why physics, chemistry, biology, and allied subjects are regarded as the classical province of science, because no one but a madman or an animist would wish to introduce the responsibility-factor into that field. The problem as to whether psychology ought to be accepted as a science in the strict sense is, likewise, illuminated, for here is a field of interest in which the subject-matter can be profitably studied without recourse to the responsibility-factor, but that factor can be, and frequently is, adduced as a working assumption at a certain stage of the inquiry (e.g. in psychiatry). Whether a mixed discipline of this sort should be regarded as a science, or split into a strictly scientific and an extra-scientific component, is a matter upon which opinion is likely to be divided.

Similarly, light is thrown upon the common distinction between 'pure' science and the engineering sciences. An engineering science is involved in the study of situations that include a subjective factor, e.g. somebody wants to reduce the pressure of traffic by building a new bridge. The engineer must adapt his specification to take account of this subjective element; but, once the specification is approved, he proceeds on strictly scientific lines to produce the bridge specified, and he is in no danger of regarding the stresses and strains affecting the structure as subjective in the same sense as are the stresses and strains that affect the motoring public in the absence of his bridge.

The definitions clear up other problems. It becomes apparent that the scientific method is not limited in its applicability to the scientific range of subjects, for it is of the greatest utility in disciplines that are not sciences. History, economics, law, and all the other disciplines fall back on the efficient economy of the scientific method whenever it is apt to the task in hand; but this does not imply that these disciplines are scientific, because the public record or tradition upon which they rest is constructed squarely on the assumption of responsibility.

Finally, the definitions offer an interesting justification for, and a decisive criticism of, the persistently advanced theory that there is something uniquely objective about scientific work. This theory mistakenly applies the criterion to the scientist himself, who is supposed to enjoy a cold, emotion-free, impartial objectivity that sets him apart from the artist, the religious adept and the lawyer. The definitions, however, leave the scientist free to obtain his inspiration by whistling in his bath like any other normal man, and only forbid him to impute a subjective factor to the subject-matter of his study as a means of accounting for its condition or behaviour. In that severely restricted sense science is a discipline that 'excludes subjectivity'. Moreover, we can now find the true ground for the persistent feeling that there is something unique about science and its method (as opposed to its methods—which are apt to turn up in the most unlikely extra-scientific places) and the equally persistent feeling that science enjoys a unique efficiency that is related to a unique economy of method.

The definitions thus tentatively proposed are advanced to be superseded. Their utility appears to lie in drawing attention to the possibility of defining science and scientific activity in terms of the working assumptions employed as compared with those employed in other fields. The crucial point appears to lie in the question whether the rational processes of the student are working by reference to an assumption of subjectivity (however named) or not, and it follows that a subject, to be scientific, must be amenable to rationalisation without recourse to that assumption.

SETON POLLOCK

THE SCOPE OF QUANTUM MECHANICS

THE SCOPE OF QUANTUM MECHANICS

IN a recent adverse review¹ of Dr David Bohm's *Causality and Chance in Modern Physics*² by Professor L. Rosenfeld, the following statement occurs: 'The true character of the causal relations of atomic physics was fully elucidated by Bohr and Heisenberg³ thirty years ago. . . .'

Call this statement 'A'.

A suggests an unwarranted precision. Exact science can never correctly define the limitations to the validity of its most comprehensive or advanced expressions, and individual scientists never know the boundaries of their own knowledge. For, as is now well understood, the limits (in scope and accuracy) of a theory, T_1 , can only be defined in the terms and calculus of a more general theory, T_2 . If T_1 is the Quantum Mechanics (Q.M.) of 1925-28, T_2 still lies ahead. Unqualified claims on behalf of Q.M. to full coverage of some specified realm may therefore be disproven at any time in the future by a fundamental advance, whose character—if history provides any precedent—is likely to be unexpected.

In order to emphasise the need for caution it may be useful to list a number of overlapping problems in relation to some or all of which (or perhaps to special cases of these problems) it may prove that the canonical methods of Q.M. are necessarily at some point inadequate, either by failing to provide an effective approximative procedure or in some more radical manner:—

- (i) *High energy* processes (including nuclear stability and transformations, variety of particles, etc.).
- (ii) Processes where the *finite values of certain dimensionless constants* render established methods ineffective (some particle interactions).
- (iii) *Derivation of the dimensionless constants* of current theory.
- (iv) Processes possibly requiring more powerful *relativistic methods*, or the equivalent.
- (v) Certain relatively low energy transfers in *complex systems* (low temperature properties, electron plasmas, etc.).
- (vi) The three-dimensional representation of the *changing inter-nuclear relations* (detailed paths of nuclei) during chemical reactions.
- (vii) The determination of the *equilibrium and changing angles* in complex atomic and molecular systems.
- (viii) Certain *chiral* (left- and right-handed) *properties*, where current theories are awkward or obscure (particle parities, non-symmetric strain tensors in crystals, rotatory power of dispersions of molecules in various media, etc.).

¹ *Nature*, 1958, 181, 658

² London, 1957. Several favourable reviews have appeared elsewhere. An objective analysis of the De Broglie-Bohm-Takabayasi theory has been made by H. Freistadt, 'The Causal Formulation of Quantum Mechanics of Particles' *Suppl. al Vol. V (Series 10) del Nuovo Cimento*, No. 1, 1957, 1-70.

³ Max Born is not mentioned, though his suggestion, based on much earlier ideas of Einstein, of the probabilistic interpretation of the wave-function was crucial. See W. Heisenberg in *Niels Bohr and the Development of Physics*, London, 1955, p. 13 and M. Born, *Science*, 1955, 122, 675.

L. L. WHYTE

This list of realms in which boundaries to Q.M. may conceivably be found is intended to suggest that the limitations of Q.M. may not lie only in the field of 'high energy processes', as now understood. It would, indeed, be surprising if the concepts and principles of Q.M. could validly suggest where they are invalid.

Now reconsider *A*. The valid content of *A* can, it seems, be no more than a (concealed) definition of what the writer chooses to call 'atomic physics'. But if this was the intention, it is surely unfortunate, since:

1. A common usage treats nuclear physics as a branch of atomic physics.
2. The identification of (*a*) 'atomic physics', with (*b*) 'the valid scope of Q.M.', is not only vague, since (*b*) cannot be defined today, but will involve the abdication of atomic physics from all claim to be the fundamental exact science, if any realms now obscure prove to lie outside the scope of Q.M.
3. The proposed usage is etymologically inappropriate. The search for the basic 'atomic' (particulate) structure of physical systems continues in the high energy and other obscure realms.

If the above reasoning is in some degree valid, the reviewer's scornful manner is robbed of any excuse. Perhaps for the benefit of the readers of *Nature* he will explain what precisely he meant by 'atomic physics', and define the realm in which he considers that the methods of Q.M. are valid.

L. L. WHYTE

REVIEWS

WITTGENSTEIN'S REMARKS ON THE FOUNDATIONS OF MATHEMATICS

I Introduction

It will be convenient to distinguish in this review¹ between (i) the *foundations of mathematics* or *philosophy of mathematics* and (ii) *general philosophy* in the following way: the former applies to philosophically interesting differences between various parts or aspects of mathematics, the latter, in so far as it is concerned with mathematics, applies to philosophically interesting differences between mathematics and other intellectual activities. For instance, the questions 'what is a (correct) proof' or, generally, 'what is mathematics' belong to general philosophy, 'what is a constructive proof' or 'what is a predicative concept' belong to the philosophy of mathematics.

Wittgenstein's remarks are concerned both with general philosophy and with the philosophy of mathematics. The vivid and incisive language (of the original²) and the many stimulating questions apart, they make a mixed impression. The aim of this review is to draw attention to observations which seem novel and stimulating, and to bring out their limitations. More space is devoted to the latter because a reader who is interested in the topics mentioned, will turn to the book itself for details.

1.1. In my opinion, Wittgenstein's significant contributions to the philosophy of mathematics concern very elementary computations, a subject which seems to have been neglected by contemporary philosophers though not e.g. by Kant. Also Wittgenstein emphasises repeatedly those aspects of proofs which are neglected in the customary treatment by the methods of mathematical logic. These matters are described in sections 7 and 8 below. His most striking fault is that he believed that all significant philosophical problems occur at the level of elementary computations, and that he made unwarranted generalisations from this limited region of mathematics to mathematics generally. But even if one is aware of other philosophical problems, the book gives a misleading impression, because it suggests

¹ L. Wittgenstein, *Remarks on the Foundations of Mathematics*, Basil Blackwell, Oxford, 1956, pp. xix + 196, 37s. 6d. The references in the review are to the German text.

² L. Wittgenstein, *Bemerkungen über die Grundlagen der Mathematik*. Miss G. E. M. Anscombe has asked to publish some corrections to her translation. These will be found at the end of this review.

an artificial dichotomy in the foundations of mathematics, so to speak: Wittgenstein against the rest. In fact, the aspects emphasised by him are a few among many. To show this, and to put his observations in perspective, we give, in sections 5 and 6, some rough indications of research in the foundations of mathematics. Naturally, large portions are only indirectly connected with the book since their purpose is to indicate what relevant material was left out of consideration altogether by Wittgenstein.

1.1a. In sections 9-12, I discuss a number of Wittgenstein's comments on 'higher mathematics' which are, for the most part, uninformed. While it would be foolish to dismiss the book because of the poor quality of these comments, it seems desirable to deal with them to keep the record straight.

1.2. However, the bulk of the remarks is concerned with general philosophy. To give some sort of intelligible account, it is necessary to discover general themes to which these remarks are relevant. I believe there are three:

1.2a. A theorem is a rule of language and the proof tells us how to use the rule (p. 81, 39).

1.2b. The meaning of a theorem is determined only after the proof (p. 79, 31 or p. 122, 30).

1.2c. A calculation is a (psychological) experiment (p. 99, 76).

1.2d. Further, I believe the link between these themes is that they are naturally suggested by an empiricist's approach to the foundations of mathematics: I am not concerned with discussing different brands of empiricism, but with the general tendency of discussing a subject in terms of things perceived and done, in short, of facts and phenomena (not in contrast to real objects, but to introspective analysis and sophisticated abstractions used in the advanced sciences).

Wittgenstein does not accept the doctrines 1.2a-1.2c; 1.2a is criticised, e.g. on page 119, 18; 1.2b on page 77, 27, or page 165; and 1.2c is rejected, e.g. page 95, 67. Generally speaking, the fault lies in the assumption (of naive empiricism) that there is a sharp distinction between the empirically given and the means of description, while according to Wittgenstein (p. 173) we need concepts to tell us what are the facts. These matters are described in section 2.

N.B. In the sections concerned with general philosophy I go by impressions and quotations out of context. Perhaps a professional philosopher will examine my case on the basis of more substantial research than I have put into this review. Also it should be observed that, below, Part I of the book is barely considered because it is closely related to Wittgenstein's *Philosophical Investigations* (for an exception see, e.g. p. 11, 27).

1.3. What has been described so far is not at all like the popular impression of 'Wittgenstein's philosophy' such as his anti-metaphysics, his panaceas of *rule of language* and *application*, his attitude

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to traditional schools of philosophy. Certainly, the book is not free from these traits of Wittgenstein's writings: but I do not believe that they are of any significance, and, in point of fact, when he embarks on serious analysis such as mentioned in 1.1 (and in sections 7 and 8 below), there is no trace of them.

1.3a. Consider the argument against the notions of mathematical *object, necessity, truth*, (e.g. p. 60, 10 or p. 142, 16): they are supposed to be metaphysical and to get in the way of straight thinking like alchemy.¹ Even if this is granted, what about the reduction to rules of language or rules of grammar? A grammatical assertion such as "soon" is an adverb' is established by an argument, it is not a simple choice once the notion of an adverb has been introduced, and similar problems arise here as with mathematics. And when one really does not understand the content of a mathematical proof or assertion, one does not know what to count as its application (p. 165, line 3) or even where to look for it (cf. 6.3 below). The effect of Wittgenstein's use of 'application' is really to exclude introspective analysis, and I am not convinced that this is right in the problems which interest him specially (e.g. p. 120, 26).

1.3b. He regarded the traditional aims of philosophy, in particular of crude empiricism, as unattainable. He objected to a mathematical foundation of mathematics because the concepts used in the foundation are not sufficiently different from the concepts described (p. 171, 13) and, he thought (p. 177) that there are no mathematical solutions to his problems. He said (p. 171, 13) that the aim of a philosophy of mathematics should consist in a clarification of its grammar (though I have not found a single passage in the book which I could with certainty identify as a 'clarification of grammar').

These general conclusions are criticised in sections 3 and 4.

1.4. I give a table of contents of this review to help those readers who get lost among the many and diverse topics which have to be mentioned if an even remotely adequate idea of the content of this book is to be conveyed.

General Philosophy

	Sections
Limitations of Empiricism	2

Philosophy of Mathematics

Criticism of Wittgenstein's general views	3-4
Background material	5-6

¹ Another objection cites the misleading (mystifying) pictures that may be associated with these notions (e.g. p. 36, 119). Equally superficial objections can be made to 'rule of language': is it made by a secret body? Is it renewed?

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Philosophy of Mathematics (cont.)

Strict Finitism	Wittgenstein's contributions	7
Proof Theory		8
Specific topics in higher mathematics		9-12
I also add a personal note		13

2 *Limitations of Empiricism*

The present section has the modest aim of suggesting a framework for reading Wittgenstein's remarks on general philosophy. For lack of a true understanding of the matters involved, I shall employ concepts commonly used in philosophy, by Wittgenstein and others, although I know how superficial similar-sounding concepts are when applied to the subject matter about which I have thought, e.g. sections 5-8 below.

Wittgenstein's starting point is this: he is not prepared to use the notions of mathematical object and mathematical truth as tools in philosophy. Actually he gives some arguments against them, but, as stated in 1.34, I do not find them convincing. To me the real objection to these notions is that, at any rate as far as I know, there does not exist a single significant development in philosophy¹ based on them; in fact, some uses of these notions seem quite contentless such as the familiar 'explanation' of the consistency of our mathematical results by saying that our results agree because we deal with the same objects. As I see it, the position is similar to that of the notions of the atom or absolute simultaneity at the time of the Greeks: neither of them could be used for understanding the world at the stage of technical and conceptual development of that time. (I chose the atom as an example of a notion with a future, and absolute simultaneity as an example of a notion which was later discarded.) In other words, the notion of a mathematical object is defective because one has no clue for using it to provide satisfactory answers to the (philosophically significant) questions which it should answer, but no case has been made out that it cannot do so—rather like philosophy itself.

Now, granted that such 'metaphysical' notions as that of a mathematical object are to be avoided in favour of an empiricist approach in the sense of 1.2d, it seems quite natural that Wittgenstein should be concerned with the positions 1.2a, 1.2b, 1.2c. Partly their attraction is due to wishful

¹ The content of this notion does not seem to lie here at all, but in the conception of set which it suggests (cf. section 2.2). The inconsistency of the naïve notion of a set is no more a defect in the concept of a mathematical object, than the Greek conception of a star as a hole in the sky is a defect in our conception of a star as a hot gas. Incidentally, it should be noted that Wittgenstein argues against a notion of a mathematical object (presumably: substance), but, at least in places (p. 124, 35 or p. 96, 71, lines 5 and 4 from below) not against the objectivity of mathematics, especially through his recognition of formal facts (p. 128, 50).

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thinking, but also there are valid, albeit inconclusive, reasons in their favour. I begin with the former.

As to 1.2*a*, it is clear that language is something 'tangible', not a 'hidden' object; as to 1.2*b*, proofs are recognised spontaneously, like colour (cf. p. 96, 70 or p. 125, 36), in contrast to theorems which, when regarded as assertions about mathematical objects, are inaccessible; as to 1.2*c*, experiments are something practical and they tell us facts (p. 99, 76 or p. 171, 14). Moreover, from these points of view the problem of an ultimate justification for mathematical assertions does not arise: in case 1.2*a*, since there is no sense in speaking of the truth of a rule anyway; in 1.2*b*, since there is no sense in speaking of the truth of an assertion before its meaning has been fixed, and so, if the proof is needed to determine the meaning of a theorem, then there can be no question of a justification for principles of proof; and in 1.2*c*, one is simply seeing what is happening and this is supposed to be unproblematic.

Of course, these formulations are oversimplified, and the positions 1.2*a*–1.2*c* are not plausible just because it is obvious that there are real problems connected with the justification of principles of proof. But it is interesting to see directly where these positions go wrong. Wittgenstein's remarks on this will be briefly touched in 2.1–2.3. Also I shall try to indicate roughly what is sound about these positions.

Note that 1.2*a* and 1.2*b* are more or less complementary because 1.2*a* leaves open the exact rôle of proof in mathematics, while 1.2*b* is mainly concerned with this question. Position 1.2*c* is contradictory to the others. What is common to them is that, separately, they are in the direction of an empiricist approach.

2.1. In favour of 1.2*a* we can say that, whatever else a theorem may be, it certainly is also a rule of inference: e.g. if A is a theorem, $A \rightarrow B$ is taken to imply B . Regarded as a reduction, 1.2*a* is open to the objection of 1.3 above. Wittgenstein stresses (p. 184, 32) a more interesting point which shows a general limitation of crude empiricism: while he regards 'believing oneself to follow a rule' as an empirical notion, the notion of following a rule correctly is not. In short, there is a non-empirical residue in the notion of a rule of language. The same applies also to the formalist conception of mathematics as a manipulation of symbols because the mathematical content does not lie in the physical production of the symbols, but in the formal fact that the symbols are produced in accordance with the given rule, or, as Wittgenstein prefers to put it (p. 81, 38), in our accepting the sequence of symbols as an application of the rule. He goes on to say 'but once we go in for this business of acceptance, then it need not be of a geometrical nature', i.e. it need not be restricted to construction of sequences of symbols according to syntactic rules. The point seems to me of importance for foundations generally, but Wittgenstein does not develop it.

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Position 1.2a raises the question why proofs are needed since a rule of language, as ordinarily understood, is a matter of simple decision. Wittgenstein does not give a plain answer, but suggests (p. 77, 28) that the proof tells us how to use the rule.

2.2. A parallel development to this suggestion is provided by 1.2b, e.g. p. 144, 4: before the proof is given the concept is pliable. The doctrine that the proof determines the meaning of the theorem, is familiar from intuitionist writings. As Gödel has pointed out to me (but see also p. 93, 61) the doctrine is supported at the level of computations where one considers symbolic operations with numerals in contrast to assertions about numbers considered as characteristics of sets: ' $5 + 7 = 12$ ', at this level, means that this equation is the last of some sequence of equations obtained by the application of certain rules, and the proof goes just the one step further of exhibiting this sequence. But as soon as one regards numbers as characteristics of sets one can meaningfully ask whether certain computational rules are correct, and to this extent statements about numbers have meaning independent of the rules of proof considered. Quite generally, it is simply not true that proof is primary and theorem derived, that only the proof determines the content of a theorem. In fact, Wittgenstein is wrong in saying that generally we change our way of looking at a theorem during the proof (p. 122, 30), but equally often we change our way of looking at the proof as a result of restating the theorem; e.g. if we are accustomed to the principle of proof that the totality of all subsets of a set is itself a set, we may reject it when it is pointed out to us that it is only valid for the notion of a combinatorial set and not, e.g. for the notion of a set as a rule of construction. (I chose this example, instead, e.g. cancellation by zero, because in the present case a restatement of the theorem is involved.)

2.2a. I believe that Wittgenstein's violent dislike of the consistency problem is connected with the thesis that *proof* is the fundamental concept. For, on the one hand it is difficult to understand how it is that different correct proofs do not lead to contradictory results, and that the calculations of different people agree. On the other hand there seems to be no approach to this problem on an empiricist basis. Wittgenstein proceeds as follows. He minimises the importance of consistency (p. 105, 81) by saying that he could imagine people who would be proud of a contradiction: but why should one attach more weight to Wittgenstein's imaginings than to the fact that people are not? He attributes (p. 122, 30) the agreement between different people to a similarity of training, though presumably the same could be said about the agreement between reports of the same physical event by different people; or (p. 13, 35) simply calls the agreement interesting, obviously implying that one should recognise this as a necessary condition for mathematics (p. 164) and not ask for explanations.

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The situation is quite unsatisfactory. We shall return to a more detailed discussion of consistency in section 11.

2.2b. Gödel's remark in section 2.2 is a particular case of a general approach to the relation between *proof* and *theorem*, which has been developed by him. (The discussion below is a slight elaboration.)

The proof is not said to *determine* the meaning, but to *enter into* the meaning of a theorem as follows: the assertion A is interpreted as: A has (actually) been proved, say by a particular individual. On this interpretation, trivially A is false before a proof is given but of course not necessarily absurd. The fact, that A is false, is not of mathematical interest, that A is true is, but from this we can only conclude that therefore it always will be true, not that it always was. Clearly this is not the common interpretation of mathematical assertions.

What has been said so far is of course clear. But significant results are obtained if the interpretation is carried through thoroughly, for then a theorem becomes an assertion about the actual structure of its own proof. For this one interprets $B \rightarrow C$ as: it has been proved that if B has been proved then so has C , and similarly with the other logical connectives. (Absurdity is used only if we have a restriction on proofs which excludes proofs of a formula A , so that for each A we may assert $A \rightarrow A$ on the present interpretation.) In propositional reasoning the following stipulation about the individual whose proofs are considered must be made: at each stage he keeps his set of theorems closed under *modus ponens*. Note that the interpretation of the symbol ' $\rightarrow$ ' is not circular, because the '*if . . . then*' in '*if B has been proved then so has C* ' applies to propositions about a concretely specified totality, namely the proofs actually constructed by the individual at a given time, and so the truth functional interpretation is quite unproblematic here. Note also that only a minimum of conditions is imposed on the proof procedures employed by the individual considered.

For more sophisticated kinds of reasoning, where as, e.g. in number theory substitution of arbitrary numerals is permitted, it is unlikely that the necessary closure conditions on the set of theorems can actually be realised, because the set would be infinite. However it seems plausible that the interpretation applies if ' *B has been proved*' is replaced by ' *B can be proved from the theorems already constructed by means of (suitably chosen) decidable rules*' so that the logical connectives in the interpretation remain unproblematic. An exact study of this would seem to be desirable.

To repeat: we have here an interpretation of (a certain class of) mathematical assertions according to which the latter are 'about' empirically given sets of proofs satisfying certain closure conditions. It is not the only, or even most natural interpretation. No doubt the phrase 'the concept is pliable before the proof is given' is elastic enough; but it is difficult to say whether it will stretch to cover the present section.

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2.3. Wittgenstein does not support position 1.2c at all (p. 124, 35 or p. 173, line 5 from bottom). His criticism of 1.2c leads to a general criticism of naïve empiricism: one needs concepts to tell us what are facts and mathematics provides them and handles them. Because of the general implications of this observation I do not feel competent to discuss it in detail, nor do I know how closely it is connected with the familiar claim that concepts are needed in the description of nature. But Wittgenstein's presentation of this view seems specially vivid, particularly in his discussion of the difference between measurement¹ and experiment (p. 98, 74): the same physical acts constitute in one case a measurement, in the other an experiment.

2.3a. The fact just mentioned immediately raises the question wherein this difference lies. It would be wholly barren to reply that it lies in the intention of the agent and that it is to be discovered by asking him whether he is making an experiment or a measurement. Wittgenstein wishes to remove the distinction from the sphere of psychology by saying that whether an act is a measurement or an experiment depends on the language game of which it is a part. It is not at all clear to me that this nice word 'language game' really clarifies the traditional term 'meaning', and does not merely replace it.

There seems to be something psychological in the distinction considered, because, with our present experience of machines, it does not seem possible to say significantly of a machine that it is making a measurement or an experiment: instead, we use it for such a purpose (cf. p. 133, 2: does a calculating machine calculate?).

Wittgenstein employs several other notions with a similar psychological flavour in his attempts to characterise (certain aspects of) proofs, e.g. p. 14, 39: to impress a procedure upon someone; p. 18, 53: to use something as a picture; p. 72, 14: to use it as a paradigm; p. 45, 153: to take in a structure at a glance, etc.

2.4. This completes a very rough review of Wittgenstein's remarks on general philosophy. My impression is that he has not put forward a coherent framework within which one can significantly discuss the nature of mathematics. We are left with a shambles, although the remarks

¹ He also talks about the difference between calculation and experiment (p. 95, 69). The discussion should be elucidated; first, it would be more reasonable to confine attention to the difference between calculating and experimenting in *mathematics* since otherwise the most obvious differences of subject matter obscure the differences at issue; second, Wittgenstein also compares calculating with measuring and with setting up means of measurement. The latter is at least as problematical as calculating: consider, e.g. the theoretical problems involved in setting up a unit of time which, contrary to popular misconceptions, is not done by setting up some conventional piece of apparatus and *defining* it to be the measure of time.

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contain some sound criticisms of the naïve empiricist approach in the sense of 1.2*d*.

The remainder of the review, except for a brief digression in section 3.3, is concerned with the foundations of mathematics in the sense of section 1.

3 Wittgenstein's General Conclusions

I consider the conclusions which I have attributed to Wittgenstein in 1.3*b*, confining myself mainly to points of specifically mathematical interest. I do not accept his conclusions since I do not think that they are fruitful for further research. The points raised in the present section and the discussion of 8.4 below seem to me to show that *the value of the book does not lie in a new point of view, but in penetrating observations and questions on a limited subject matter.*

3.1. Even though the aims of the traditional schools of philosophy in their crude form are unattainable, I do not see why these aims should not be modified in the light of criticism, and then pursued. Thus, though the requirements of strict empiricism cannot be satisfied, a leaning towards it seems fruitful. It seems to me that Lorenzen's operative logic constitutes an interesting formulation of the idea that theorems are rules of language: Lorenzen, of course, does not try to reduce the act of following a rule (correctly) to empiricist terms, but starts with the fact (or: idealisation) that we do follow rules. And notwithstanding the criticisms of 2.2*a* and 2.2*b*, we can develop a mathematics based on the primitive concept of *proof* or even *constructive proof*, as the intuitionists have done (6): we ignore the problem of how we come to recognise a proof and start from the fact (or: idealisation) that we do. Only, as stated in 2.1, it is not likely that one approach will turn out to be 'more unique' than the other (cf. also 8.3*b*).

3.2. As far as mathematical foundations are concerned, it is certainly true that if mathematical concepts are used in foundations, they are liable to raise the same type of problem as they are supposed to answer: we are left with elucidations rather than explanations (see Gödel on consistency proofs, impredicativity of the notion of intuitionistic proof, and of a set: to mention difficulties met by the three most prominent mathematical approaches to the foundations of mathematics). But to quote out of context (p. 174, 16): don't demand too much and have no fear of your problems dissolving into nothing. In fact, mathematical logic has done far more to get an overall view of mathematics, to help us find our way about (p. 104, 80), than any other single discipline: it provided concepts necessary for the description of mathematics, just as, according to Wittgenstein, mathematics provides the concepts necessary in the description of nature.

Wittgenstein's views on mathematical logic are not worth much because he knew very little and what he knew was confined to the Frege-Russell

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line of goods.¹ But it is true that the methods of mathematical logic have not been applied successfully to the subject of elementary computations (7.1), and this is precisely the subject which interested him most.

3.3. As to Wittgenstein's conception of philosophy, there are two questions: the rejection of other conceptions and the promise of his own, the clarification of the grammar of mathematics. He does not say exactly wherein this consists, but it is evidently to do with applications and intellectual institutions (e.g. p. 176, 18 last paragraph, or p. 173, paragraph 8). The concepts used in his description of our mathematical activity are those of 2.3*a*. Now, I see no objection to them on formal grounds, and I can believe that they afford a better framework than either a purely empiricist (behaviourist) or a purely introspective analysis. But these notions and the whole programme of clarifying the grammar remind me of the 'soft' options² at school: human geography (not the mineral composition of mountains, but their effect on history) or economic chemistry (not the atomic structure of the chemical elements, but their uses in society). It is all sensible and interesting, not hackneyed (p. 170), but often bypasses the problems which later turn out to be most fruitful. But even if this conception turns out to be useful, there is no clear reason for rejecting the others, no more than that human geography should exclude scientific geology. There are such traditional problems as the genesis of our mathematical concepts, the justification of proofs, i.e. what makes them correct rather than what makes them interesting, which are the fundamental concepts and which derived: we need a conceptual apparatus to formulate these questions in a satisfactory way, and we do not have such an apparatus. But it seems unlikely that the concepts favoured by Wittgenstein will provide it. It is by no means clear that these questions are ripe for a precise formulation, any more than the general (and natural) questions of present day mathematics were ripe for a formulation at the time of the Greeks.

4

There is another, less austere conception of the philosophy of mathematics, which Wittgenstein ignores too. Since this conception, it seems to me, underlies most of current work in mathematical logic, he implicitly rejects it by rejecting mathematical logic. As mathematics has grown, a variety of different methods of proof, definitions, theorems have

¹ This is not at all typical of the subject. However, I am told that many professional philosophers are similarly uneducated and are, therefore, likely to have similarly distorted views.

² This impression of philosophy is supported by Wittgenstein's general comments on his conception of philosophy: perhaps one should not pay too much attention to them since some passages in the book (cf. Sections 7 and 8 below) involve 'hard analysis'.

accumulated. By the light of nature we see differences, groupings within one branch, and similarities between different branches of mathematics. One may see one aim of a philosophy of mathematics in getting a clear understanding of these connections, and there is no reason in advance why this should be done only by reference to 'applications', and not, e.g. by mathematical properties, by mathematical characterisations. From this point of view it is a contribution to the philosophy of mathematics if a new aspect of the methods of mathematics has been noticed, such as, e.g. Wittgenstein's own observations discussed in sections 7 and 8 below; here there is no one fundamental problem. I regard the 'rival' philosophies of mathematics in this light: not as contradictory in substance, but as emphasising different aspects of mathematics (cf. writings of Bernays quoted below).

4.1. It should be observed that the originators of the rival philosophies took a different view: they insisted on rejecting those aspects of mathematics which they did not consider themselves; like a woman who fears one would not be interested in her if one remembered that others existed besides her. Of course, from the point of view of a *Lebensphilosophie* these rival philosophies are contradictory because they regard different aspects of mathematics as specially important.

5 Background (Set Theory)

The historical background to Wittgenstein's remarks consists of two parts. First there is the logicistic reduction of mathematics to logic and abstract set theory: many of Wittgenstein's remarks are a reaction against this. Second, there are the so-called constructive tendencies which are also a reaction to the logicistic approach or, at least, are in a different direction. We describe the reduction to set theory first, and then some brands of constructivism in order to compare Wittgenstein's views with them.

Abstract set theory provides the most famous of all foundations of mathematics. The remarkable fact is this: each known branch of mathematics has a model in abstract set theory, and frequently, a most natural model. Thus, e.g. the question 'what is a number' to which it is hard to give a natural meaning, gets the answer: an element of the set which is the intersection of all inductive sets. Similarly, the corresponding questions for other mathematical concepts are answered in this uniform way. It is fair to say that this programme of regarding our mathematics from this point of view of set theory has been carried out in far more detail than, e.g. the programme of describing the physical world as assemblages of fundamental particles. (The common feature of these programmes is that they reduce the *number* of 'primitives'.) In particular, the development of arithmetic within set theory has not only helped us to understand

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arithmetic better, but has had important repercussions on the study of set theory and logic generally, e.g. it permits the application of Gödel's incompleteness theorem to systems of set theory, and related results.

5.1. The interest of the discovery just described cannot be doubted. But it is not so clear that it satisfies the philosopher who seeks a 'simpler' foundation, who looks for the fundamental concepts in mathematics and wants to build up derived ones. Those philosophers who see the content of a scientific statement in the observational verification will deny that the fundamental particles are simpler than the physical objects of our experience, but they cannot deny that at least they are smaller. There is no obvious order in which abstract sets precede numbers, as the fundamental particles precede objects of our immediate experience in size. And, as e.g. Poincaré pointed out with great lucidity, the reduction of arithmetic to set theory itself requires the processes of arithmetic. If the notion of number presupposes the concept of a finite set, even then, as Lorenzen has observed,¹ this does not mean that one has to consider arbitrary sets first and then restrict them to finite ones. In other words, in this search for foundations, for notions with a more elementary content, one may wish to select parts of mathematics, or, in particular, parts of set theory. Wittgenstein emphasises strongly the mathematical significance of such selections.

5.2. Another respect in which the set theoretic foundations fail, is in characterising the constructive aspects of mathematics which Bernays calls, epigrammatically, a mathematics of doing (cf. also p. 118, 15) in contrast to a mathematics of being, or, more formally, an idealisation of process as against an idealisation of what is the case. Neither of these conceptions can be expected to be fully comprehended by the other; although there are interesting formal relations between them. It is interesting to note that there is not only a constructivist critique of so-called classical ('platonistic') mathematics, but also a converse. In fact, many mathematicians are almost proud to declare that they don't understand intuitionism (or the other constructive tendencies); they ask: how do you define constructive proof? and do not really expect an answer; rightly as long as they presuppose a definition in set theoretical terms. Their complaint about the vagueness (meaninglessness) of the notion of constructive proof is on a par with the intuitionist complaint about the meaninglessness of the notion of arbitrary functions: how do you specify them, since they are supposed to be non-enumerable? Yet without giving a list of either arbitrary sets or of all constructive proofs, Zermelo laid down properties of the former notion in his set theory and Heyting of the latter in his axiom systems for intuitionistic logic. From now on we shall be mainly concerned with the constructive side and we discuss it on its merits without attempting to reduce it to set theoretical terms.

¹ *Math. Ann.*, 1951, 123, 331-338

6 Background (Constructivity)

It is the custom to lump together all constructive tendencies under the heading of *intuitionism*. To get a little more orientation we shall distinguish between intuitionism proper as developed by Brouwer and Heyting, and *finitism*. There is an even narrower conception of constructive mathematics, namely *strict finitism*, a notion described by Bernays in 'Sur le platonisme dans les mathématiques'.¹ Wittgenstein's views seem related and favourable to intuitionism, probably mainly because of common features such as the objection to the idea of a mathematical object, the priority attached to proofs over theorems (cf. 2.2), and the use of Brouwer's household example of the decimal expansion of π (p. 138, 9 or p. 144, 19, or p. 147, 27). But a closer look shows that this similarity is superficial, and that Wittgenstein's views on mathematics are near those of strict finitism; or, perhaps one should say, he concentrates on the strictly finitist aspects of mathematics. To justify this assertion it is necessary to describe some differences between these three constructivist tendencies. (Strict finitism will be considered in the next section.)

6.1. First, we describe differences on a general (epistemological) level. Hilbert-Bernays² states that intuitionism permits the use of general logical considerations in addition to the combinatorial facts with which finitist mathematics is concerned. Intuitionism deals with 'mental constructions' while a finitist piece of mathematics is regarded as a Gedanken experiment (description of an experiment) with concrete objects³ which are thought of as reproducible, are to be recognisable, and surveyable, i.e. thought of as built up of discrete parts whose structure can be surveyed.⁴ Intuitionism includes finitism because a picture of a concrete object can be used in a mental construction, as this word is employed by the intuitionists. But it goes beyond finitism because it makes statements concerning all possible constructions, which certainly do not constitute a concrete totality. These, and similar, general differences become apparent in the typical features of the notion of intuitionist as opposed to finitist proof: a false proposition implies anything; undecided propositions, and even implications between such propositions, may be used as premisses in implications, i.e. one makes assertions which involve an hypothetical proof, namely a proof of the premise, though the totality of all proofs is not concretely specified (it is regarded as an accident that in most proofs of implications $A \rightarrow B$ the details of the proof of A are not used in the proof of B , as, e.g. in $[A \ \& \ (A \rightarrow B)] \rightarrow B$ where one need merely attach the proof of $A \rightarrow B$ to the

¹ *Enseignement mathématique*, 1935, 34, 52-69

² *Grundlagen der Mathematik*, 1934 I, 43

³ *Ibid.*, p. 20, line 8 from foot.

⁴ *Ibid.*, p. 21, lines 21-23, where Bernays uses 'überblickbar' instead of Wittgenstein's 'übersehbar', e.g. p. 65, 1, with its ambiguity of 'overlook' and 'look over'.

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proof of A in order to get a proof of B, though, in fact, Brouwer's proof of the fan theorem is the only known example of the contrary case); as a result it is not clear what 'construction' constitutes the content of such an implication: this applies in particular to the double negations of which intuitionist writings are full.

6.1a. Granted that features of this kind are typical of the difference between finitist and intuitionist mathematics, it is clear that it is unprofitable to compare Wittgenstein's views and intuitionism. For, in his simple computational examples we are dealing with strictly combinatorial processes, and the typically intuitionist concepts do not apply: he leaves off before intuitionism starts.

6.2. Finitist mathematics does not use the general notion of a constructive proof at all, in fact it might be said to avoid *logical* inferences (which involve an impredicative concept of proof) because it is restricted to purely combinatorial operations. In particular, the logical connectives have a purely combinatorial character since they are applied only to decidable formulae and so the truth functional interpretation (truth table method) is not problematic. Universal quantifiers are not used at all except in so far as they can be replaced by free variables, e.g. not in the premiss of an implication. Existential quantifiers are used as shorthand for a (constructive) function or functional: if they occur in a premiss, e.g. $(\exists x) A(x) \rightarrow B$, they are interpreted as $A(n) \rightarrow B$, when n is a free variable which does not occur in $A(x)$. Iterated implications are not used at all. These restrictions all follow more or less cogently from the general conception of a mathematics about concretely presented objects.

6.2a. The reason why there is such general confusion about the difference between finitist and intuitionist proofs is simply this: one very rarely uses all that intuitionism would allow, e.g. up to the present we do not know, for A known to be recursive, a single intuitionist proof of $\sim(x) A(x)$ where we cannot specify an n such that $\sim A(n)$ (though the general conception of an intuitionist proof makes it plausible that there are such A); again $(x) A(x) \rightarrow (y) B(y)$ can always be sharpened in known cases to $A[f(n)] \rightarrow B(n)$ where $f(n)$ is a specified (constructive) function. Iterated implications are extremely rare in mathematics anyway.

6.3. In short, all the mathematics which Wittgenstein considers clear (not, e.g. the completeness of the set of real numbers) fits comfortably within the framework of finitist mathematics, and so, as stated in 6.1a, it is futile to compare his views with intuitionism. In fact it will turn out that an even narrower aspect of mathematics is considered by him.

7 Strict finitism

Finitism is, of course, an idealisation too, i.e. it ignores certain differences and distinctions based upon them. In particular, it does not distinguish

here because, by the methods of logic alone, we could not decide whether a particular formula of logic corresponds to some given formula of numerical arithmetic. Also there would always be the question of whether a rule has been correctly applied (p. 89, 53, or p. 91, 56).

8.1a. This point seems wholly acceptable, and, what is more, quite familiar: it concerns the metamathematical methods used for investigating relations between two systems. We do not speak of a 'reduction' unless the metamathematical methods are weaker in some suitable sense or, at least, more evident than the methods studied. For instance, in his reduction of formalised classical arithmetic to intuitionistic arithmetic, Gödel was careful to use finitist methods of proof. Again, if a system is decidable, i.e. if there is an effective method of associating with every formula of the system one of the formulae $0 = 0$, $0 = 1$ ($0 = 1$ with provable ones, $0 = 1$ with the others), we do not generally speak of a 'reduction' of the system to arithmetic modulo 2: for, in general, the method used for constructing this translation goes far beyond arithmetic modulo 2. In the countless language of professional logicians one replies to Wittgenstein's question how we know that the rule (of translation) has been correctly applied by saying: the whole reduction must be considered relative to the methods of proof used in the metamathematical argument. And one would agree with him that the metamathematics required for a translation of arithmetic into logic requires some arithmetical concepts. However one would remember that without being a reduction such a translation can be of central importance (cf. end of section 5).

8.1b. Wittgenstein's next point is much more positive. Instead of just saying that the metamathematical methods used in the translation include the methods studied, he looks for appropriate weaker methods, and observes, e.g. that the reduction of the decimal notation for numerals to the stroke notation (p. 66, 3) cannot be done by strictly finitist methods. He thereby uses the notion of strict finitism for making a natural distinction, which is not made customarily.

8.2. Wittgenstein repeatedly raises the question of characterising the equivalence of proofs in contrast to equivalence of results (p. 69, 8; p. 69, 9; p. 66, 3 last paragraph). This is a somewhat elusive notion, a little like Heyting's notion of the completeness of a calculus with respect to proofs and not only with respect to results,¹ but in particular cases it is clear enough. For instance, Shoenfield² showed how to replace the induction schema in the elementary quantifier-free arithmetic of addition by means of a finite number of axioms, including $a + b = b + a$, without altering the set of theorems. Now, it is intuitively clear that the proof of $a + b = b + a$

¹ Cf. his book *Intuitionism. An Introduction*, p. 102.
² *Journal of Symbolic Logic*, 1957, 22, 112.

between constructions which consist of a finite number of steps and those which can actually be carried out, or between configurations which consist of a finite number of discrete parts and those which can actually be kept in mind (or surveyed). In fact within any given degree of sophistication of proofs a classification according to the degree of complexity is most natural. Wittgenstein stresses (p. 65, 2) the further point that explicit definitions and new notations may convert a piece of mathematics which is not strictly finitist into one.

7.1. No rigorous work has been done on this subject, partly, no doubt, because the current methods of mathematical logic do not seem to lend themselves to it. Perhaps the study of finite computing machines or the human learning process will make the problems which arise here practically pressing. But also, so far, this concept of finitist proof in the strict sense has not been applied to questions which interest logicians such as the general notion of 'equivalence of proofs' or 'content of proofs'. It seems to me that Wittgenstein gives some very interesting hints in this connection which will be described in the next section.

7.2. To avoid misunderstanding: I myself have not worked on strict finitism, and do not know what direction research on it might take. But Bernays and Wittgenstein have certainly drawn attention to a new *area* of research in foundations. Those logicians and philosophers whose taste leads them to work in developed fields where others have created concepts on which they can model their own, will certainly not be attracted to strict finitism at the present stage; just as they would not have been attracted to intuitionism before Heyting analysed its formal structure and Tarski important aspects of its interpretation. Hao Wang continues this discussion in a forthcoming article in *Dialectica*; in particular Wittgenstein's concern (para. 2.2) with the relation between proof and theorem is seen in a new light if one broadens the notion of strict finitism to *that which one can actually grasp*; for, after one has gone through a proof one actually handles a theorem differently from before. The suggestion is undoubtedly in the right direction.

8 *Equivalence of Proofs*

I shall discuss Wittgenstein's views by reference to his criticism of the reduction of arithmetic to logic and to some remarks of his on non-constructive existence proofs in analysis. It seems to me that he has to say about the reduction of arithmetic to logic is not concerned with any peculiarities of the branches of mathematics considered, but applies generally to *proof theoretical reductions* or *translations*, either mapping of proofs of one kind into proofs of another, or theorems (provable formulae) of one system into theorems of another.

8.1. Wittgenstein's first point concerning the reduction of numerical arithmetic to logic with identity is this: we do not really have a reduction

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by induction does not have an 'equivalent' counterpart in the finitely axiomatised system.

8.2a. Wittgenstein does not succeed in characterising this notion of equivalence or in comparing¹ proofs. He recognises (p. 93, 60) that under too strict a criterion of equivalence a proof will be equivalent only to itself: he does not see any objection to this, and does not attempt to find fruitful criteria. But, in my opinion, he raises an interesting problem here.

8.3. He does attempt to find a characterisation of a very general sort by basing a comparison of proofs on the application,² or, as he puts it (p. 155, 46), on what I can do with it. I believe that at a certain level this is a useful approach, but its limitations are more interesting.

8.3a. Wittgenstein makes the remark in connection with non-constructive existence proofs in which I myself have been specially interested. Suppose we have a proof of $(\exists x)A(x)$ or $(\exists x)(y)B(x, y)$ where we think of the variables as ranging over natural numbers. The first thing that I may expect to 'do' with the proof of such a formula is to read off from it instructions for calculating an n such that $A(n)$ holds. (With the ordinary methods of proof this can be done if A is recursive.) In general I cannot 'do' this in the second case, e.g. if we have a proof (naturally by *reductio ad absurdum*) of $\sim(x)(\exists y) \sim B(x, y)$. Such a proof would generally proceed as follows: suppose $(x)(\exists y) \sim B(x, y)$, then for a certain function $Y(x)$, $(x) \sim B[x, Y(x)]$; the proof (in general) shows $\sim(x) \sim B[x, Y(x)]$ by constructing explicitly an x_y (depending on Y) such that

$$B[x_y, Y(x_y)]. \quad (1)$$

This formula, with its construction of (the functional) x_y in terms of Y , may be said to tell us what we can 'do' with the non-constructive existence proof.³

I do not think that this analysis is at all artificial: but it certainly presupposes that we are looking for something of this sort, i.e. that we wish to express 'what we can do with the non-constructive proof' in terms of some quantifier-free (possibly finitist) assertion like (1) above. Perhaps

¹ An incidental observation: I have sometimes felt that Wittgenstein's violent dislike of the notion of mathematical truth is connected with comparisons of proofs. He could see that the comparison of proofs was informative, and he didn't have a snappy answer to the objection sometimes made by mathematicians: But all we want to know is whether the theorem is *true*. The answer is: If you think about what you are saying you will see that you are wrong.

² There seems to be a conflict with p. 157, 52, where he says that it is useless in the philosophy of mathematics to reformulate proofs: one would have thought that this might exhibit the application especially clearly.

³ All this involves a restatement of the theorem and a consequent reformulation of the proof without, as one says, changing the idea of the proof: cf. the preceding footnote.

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it is particularly easy to say what we can 'do' with a non-constructive existence proof because the matter of constructivity is at issue, and so a complete elimination of the non-constructive use of the quantifier is all we want, and we get it. The situation appears even simpler *post hoc* because also from a 'non-constructive' classical point of view, (1) is better than $(\text{Ex})(y)B(x, y)$ in the following sense: $(\text{Ex})(y)B(x, y)$ follows from $B[x_y, Y(x_y)]$ with the variable Y by pure (classical) logic, but not the converse: graphically, (1) is more informative than $(\text{Ex})(y)B(x, y)$.

8.3b. But it seems to me that here we have a very special case in the foundations of mathematics, because the requirements on a satisfactory solution are pretty clear: as we have said above, the adjective 'non-constructive' itself suggests what we should aim for. And in any case, what we can 'do' with the non-constructive proof is more informative than the non-constructive theorem.

But in the more fundamental problems of the foundations of mathematics, the question 'what can I do with it' does not seem to help. Suppose I have a finitist and a non-finitist proof of a universal formula $(x)A(x)$: What can I 'do' with the former which I cannot do with the latter? In both cases, for each numeral n (errors excepted) we shall have $A(n)$. Incidentally, comparison between the intuitionist proof of the fan theorem and König's proof of the corresponding theorem in classical mathematics raises the same question:¹ what can I 'do' with one, what with the other? Nothing like the question of 'estimates' for existential quantifiers, which is central in the cases of 8.3a, is relevant here. What one is left with is simply this: if one *starts* with the classical conception Brouwer's proof is simply not valid because he makes an unjustified restriction on the methods of proof of the hypothesis which in any case is not relevant because one only wishes to assume its truth, and if one *starts* with the intuitionistic conception König's proof is not valid because he employs a (provably) non-constructive least number operator. I do not see any 'practical purpose' or considerations of 'usefulness' which could decide between the two proofs.

8.4. It is my impression that the emphasis on 'application' or 'on what we can do with it' aims at unifying our point of view: we are to look at two conceptions like the classical and intuitionist conception of mathematics, and find a place for each from our point of view, namely: according to their applications. Mathematicians sometimes pretend to a similar criterion of judgment, namely: 'mathematical fruitfulness'. Everybody knows that in subtle cases the criterion of fruitfulness usually does not apply because people differ in just these cases in what they find

¹ The rest of para. 8. 3b presupposes acquaintance with the proofs of Brouwer's fan theorem, e.g. in Heyting's book cited in section 8.2 above, and of the Unendlichkeitslemma.

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interesting. From 8.3b it appears that in subtle cases Wittgenstein's notion of application is no better. The aim is not achieved, and this fact seems to support the conclusion of section 3 above.

9 *Higher Mathematics*

The remainder of this review deals with isolated topics which are discussed at length in the book. I make no attempt to relate them to a general point of view. Some of them are so absurd that they seem due to very general misconceptions and not just carelessness (cf. p. 55, lines 11 and 12). It might be interesting to trace such connections.

Wittgenstein says (p. 58, 6) that it was the diagonal argument which gave sense to the assertion that the set of all sequences (of natural numbers) is not enumerable. The definition is: a set of sequences $a(1), \dots, a(m), \dots$ is enumerable if there is a double sequence $s(n, 1), \dots, s(n, m), \dots$, $n = 1, 2, \dots$ with the following property: for each sequence of the set, there is an n_a such that $a(1), \dots$ is identical with $s(n_a, 1), \dots$, i.e. for all m , $a(m) = s(n_a, m)$. And the diagonal argument states, that the set of all sequences is not enumerable because (i) if $s(n, m)$ is a double sequence, then $s(n, n) + 1$ is also a sequence and so (ii) any proposed enumeration $s(n, m)$ fails to include one sequence, namely $s(n, n) + 1$. One could only wish that all one's assertions had as much sense as the assertion of the non-enumerability of the set of all sequences before its proof!

9.1. What is wrong here? Well, after all there was a paradox, Skolem's paradox, which puzzled people. The mistake is to think that the diagonal argument applies *only* to the set of all sequences: given any set of simple and double sequences satisfying (i), the non-enumerability of these simple sequences by a double sequence of the set is established. In other words we may expect quite 'small' models of a set theory which satisfy the nonenumerability condition, since not only the sequences but also the means of enumeration of these sequences are restricted in such a model. But this does not at all mean that the set of all sequences is ill defined: the fact remains this set has the property (i).

10

Wittgenstein criticises Gödel's first incompleteness theorem; or, at least, the part which states that if a suitable system of arithmetic is consistent then there is a true formula of the form $(n)A(n)$ which is not provable in the system; the formula is one with number q which states: for every n , n is not the number of a proof of the formula with number q , i.e. a proof of itself. The arguments are wild, including such points as an inconsistency wouldn't matter (p. 51, 11), or how do we know that this is the correct translation of the arithmetic formula $(n)A(n)$ (p. 51, 10), or what does it mean to suppose that a formula is provable (p. 177, bottom). Even if

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an inconsistency didn't 'matter', one cannot hope to discuss significantly on this basis a result which explicitly supposes consistency of the system. Why does he think that the elaborate details of Gödel's paper are needed? Just because Gödel has to show that on the assumption of consistency the proposed translation is correct. And finally, one of the major purposes of considering formal systems (and it is formal systems which Gödel considers) is that a clear combinatorial (geometric) meaning is given to a formula being provable.

10.1. Just as with the diagonal argument, nothing is *wrong* here, but perhaps the following explanations are appropriate.

First of all, logicians are quite aware of the choices open in translating a syntactic assertion in English into arithmetic form; in fact, Rosser's variant of Gödel's proof depends just on this fact. For the first undecidability theorem all that is needed of a translation of the proof predicate (m is — the number of — a proof of — the formula with number — n) is this: there should be an expression $\mathcal{P}(x, y)$ in the system such that $\mathcal{P}(o^{(m)}, o^{(n)})$ is provable (refutable) in the system for the numerals $o^{(m)}$ and $o^{(n)}$ just in case m is (not) a proof of n . And this is ensured by consistency and the other conditions imposed on the system. For a significant formulation of the second undecidability theorem the expression $\mathcal{P}$ has to be so chosen that one can 'see' that it possesses certain properties of the proof predicate; i.e. one must be able to prove formally in the system certain implications involving $\mathcal{P}$. This was first analysed by Hilbert-Bernays and recently Feferman has obtained interesting further results. This work shows what a workmanlike approach to such a matter as an ambiguity in translation should be: it is not regarded as an inherent defect, but it is analysed till one sees what is actually needed in a particular context.

The beauty of Gödel's own formulation is that his result can be separated from the question of truth in arithmetic. He has found a formula $(x)A(x)$, A primitive recursive, which is formally undecidable in the given system $\mathcal{S}$ if $\mathcal{S}$ is consistent. Now, given this syntactic result one argues: since any closed proposition is true or false, either $(x)A(x)$ or $\sim(x)A(x)$ is true, and so there is a true proposition which cannot be proved in $\mathcal{S}$ (on the intended interpretation of $\mathcal{S}$). One sees that all one needs of the concept of truth is that either $\mathcal{A}$ or $\sim\mathcal{A}$. Actually, our notion of truth is clear enough to argue: $(x)A(x)$ is true. For if it were false for the numeral $o^{(n)}$, then $\sim A(o^{(n)})$ would be provable in $\mathcal{S}$ (provided all recursive predicates can be computed in $\mathcal{S}$) and so $\sim(x)A(x)$ would be provable.

To me Gödel's results do not at all suggest that our intuitive notion of an integer is defective: I can't believe that anyone ever had an intuition that there was a recursive decision method for arithmetic, even if, like Hilbert, he wanted one. It is not a death blow to the axiomatic approach since we now consider partial instead of complete axiomatisations. Nor

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necessarily to Hilbert's consistency problem: one has to examine the methods employed in the consistency proof on their merits. In fact, what Gödel shows is that the methods employed in a consistency proof of a system of arithmetic cannot be more evident than the methods formalised in the system, simply by virtue of being more restrictive. However, just because of this Gödel's results did destroy Hilbert's aim of getting rid of all problems of foundations once and for all. To me, the most striking fact is this: while nobody has ever been troubled because the particular sentence of arithmetic is undecidable, so to speak because the systems are *open*, Gödel's own principle for deciding such sentences shows that they are *artificially* open: his principle is: if $\mathcal{A}$ is provable in the system then $\mathcal{A}$. This principle can be formulated in each of the usual systems of arithmetic, and yet it extends the system considered.

II

Wittgenstein's criticism of the consistency problem ranges from a proposal to use the double negation as an enforced negation (p. 53, 18) (when $\sim A$ and $\sim\sim A$ would not be contradictory), to the proposal of not drawing conclusions from a contradiction (p. 169, 12) (as described, e.g. in Ackermann's system of strict implication,¹ where the consistency problem is not relevant), to modifications in our arguments after we reach a contradiction (which is done every day, both as a correction of errors, oversights in definitions, etc.) In fact the consistency problem as conceived by Hilbert had a perfectly specific point: if one can prove, by limited ('understood') methods, the consistency of a system S , then if a universal recursive formula can be proved in S , then the same formula can be proved by the limited methods. Since he considered more elaborate formulae as 'ideal' elements, with no hope of assigning a clear constructive meaning to them,² this was the most he could expect from foundations of arithmetic. In addition, it turned out that consistency proofs gave a great deal of additional information.

II.1. Here, for once, Wittgenstein also makes a justified objection (p. 130, 56) among all the wild shots which miss the mark: the one-sidedness of the consistency problem. From the consistency we can draw some conclusions (sections 10, 11), perhaps more than one would expect. Yet, as Gödel has shown there are formally consistent systems of arithmetic, which are ω -inconsistent, i.e. $(\text{Ex})A(x)$ can be proved but for each numeral $0, 1, \dots$, $A(0), A(1), \dots$ can be refuted. This had obviously not been suspected. And even some of the results of the Hilbert school are more

¹ *Journal of Symbolic Logic*, 1956, 21, 113-128.

² Though this has been done by Herbrand and others (cf. Section 13).

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informatively described, e.g. as extended ϵ -theorems than as mere consistency results.

11.2. It is true that some popular reasons for the consistency problem are unacceptable. If, e.g. one's aim is to save 'our mathematics', but if, like Hilbert, one regards only universal formulae as significant and others as auxiliary (ideal) one has not saved anything because these formulae can be proved by the methods employed in the consistency proof itself. If one is interested in scientific applications, consistency is certainly not enough, the system must be correct for the intended application, and in this case consistency will be a by-product. (Wittgenstein says, p. 104, 80, that consistency will be a by-product if one aims at a useful application: the quantum physicists wish it were.) And indeed an inconsistency is not shattering. For, if one thinks of mathematics as a body of rules and finds an inconsistency one modifies their statement: Cantor's proofs still stand although they can be embedded in Frege's inconsistent system. And if one thinks of mathematics as concerned with mathematical objects, the progress of science shows how our basic conceptions (cf. page 138, n.) may turn out to be inconsistent with experience, yet many observations made previously still stand, and only those whose interpretation is strongly tied to the particular conception, are discarded.

Finally, as is often stressed by the physicists, if mathematics is used as a handmaiden of the sciences, an inconsistency of the rules obtained by an unintended application, even if this had not been excluded explicitly in advance, may be quite acceptable; once, a mathematician derived a contradiction from the definition of a function introduced by a famous physicist, upon which the latter commented: Did I make a mess of my function? In any case there are ambiguities and uncertainties in the physical assumptions, so why not put up with similar faults in the mathematical manipulation? In fact, as Wittgenstein observed explicitly (p. 189) one could do physics without distinguishing between mathematical and physical facts at all. In other words, we have the interesting fact that something to which our conception of mathematics does not apply, can also be used as a handmaiden of the sciences. But this fact does not invalidate our conception of mathematics which is more exacting than that just described.

When all this is recognised, the mathematical problem of consistency still stands, and is fruitful: proofs of consistency and, more generally, of independence yield, perhaps, a better control over a calculus than anything else. (In one place, Wittgenstein seems to agree with this: p. 106, lines 8, 7 from below). Also, the separation between mathematical and physical facts, even if artificial in places, has its value in the manipulation of physical theories, particularly when a modification of the theory is required. Of course, when we speak of the consistency of rules, we make a mathematical assertion, and ignore certain epistemological problems, such as the one which

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Wittgenstein stresses particularly: how do we know that the rules have been correctly applied.¹ I agree that this is also a problem. But it is completely barren within the context of consistency because it questions the very basis from which the consistency problem derives its significance. For, this epistemological problem arises with every application of a rule (and, indeed, an analogous problem arises with all communication) while the consistency problem arises with specific sets of rules.

12

Wittgenstein simply did not know what to say about the paradoxes. I don't either. But one thing is clear: the fruitful problem is not to 'get rid of them' but to get something out of them. Gödel got his general results on formal systems out of one set of paradoxes, Loeb got a beautiful result out of another.

I myself have found the following version of Russell's paradox illuminating, though it does not seem to lend itself to generalisation.

Recall first that in one definition of natural numbers in set theory, the null set corresponds to the number 0 and, if N corresponds to n , $N_{\cup}\{N\}$ corresponds to the successor of n . There is no greatest integer because $N_{\cup}\{N\}$ is not included in N .

Now let us consider any set A with the property ($\mathscr{P}$) that its members do not belong to themselves, i.e. $X \in A \rightarrow X \notin X$, and hence $A \notin A$. So $A_{\cup}\{A\}$ also has the property $\mathscr{P}$. In other words the Russell paradox involves the same argument as the theorem that there is no greatest integer, and at the same time suggests a natural way of generalising the successor construction. (From this point of view the Russell Paradox does not seem more astonishing than a child's assumption that there is a greatest integer: we have overlooked the fact that not every property has a definite extension.)

It is hard to say something really coherent about the paradoxes, but one can do better than speak of a head of Janus looking down on the other propositions (p. 131, 59), though I like the psychological accuracy of p. 131, 58.

13 Personal Note

I knew Wittgenstein from 1942 to his death. We spent a lot of time together talking about the foundations of mathematics, at a stage when I had read nothing on it other than the usual *Schundliteratur*. I realise now from this book that the topics raised were far from the centre of his interest though he never let me suspect it.

¹ What is right about his criticism is this: if one is concerned with reliability in a realistic sense, then it would be wrong to consider only the abstract rules and not their use.

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What remains to me of the agreeable illusions produced by the discussions of this period is, perhaps, this: every significant piece of mathematics has a solid mathematical core (p. 142, 16) and if we look honestly, we shall see it. That is why Hilbert-Bernays vol. II, and particularly Herbrand's theorem satisfied me: it separates out the combinatorial (quantifier-free) part of a proof (in predicate logic) which is specific to the particular case, from the 'logical' steps at the end. Certain interpretations of arithmetic and analysis have a similar appeal for me. I realise that there are other points of view,¹ but for the branches of mathematics just mentioned, I still see the mathematical core in the combinatorial or constructive aspect of the proof.

I did not enjoy reading the present book. Of course I do not know what I should have thought of it fifteen years ago; now it seems to me to be a surprisingly insignificant product of a sparkling mind.

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Here follows a list of corrections to Miss G. E. M. Anscombe's translation.

- p. 17, line 22, read 'again' for 'even'.
- p. 79, line 23, read 'of intersection of significant sentences' for 'to divide significant sentences at'.
- p. 89, line 13, read 'the following point constantly occurs to me' for 'I am constantly struck by the following'.
- p. 153, line 10, read 'definition' for 'explanation'.
- p. 160, line 13 from bottom, read 'something like' for 'as it were'.
- p. 168, line 4 read 'take the metaphysical sting out of it' for 'extract the metaphysical thorn'.
- lines 6 and 5 from bottom, read 'this way of calculating led to the loss of human lives' for 'many human lives had been lost through this way of calculating'.
- p. 169, line 7, read 'cancelling' for 'reduction'.
- line 11 from bottom, read 'destroy' for 'nullify'.
- p. 170, line 10 from bottom, read 'rigour' for 'strictness'.
- p. 174, line 7 from bottom, read 'by-pass it' for 'pass it by'.
- line 5 from bottom read 'continuously and without' for 'without omission or'.
- p. 177, line 5, read 'i.e. we' for 'and hence'.
- p. 183, line 12, read '5' for '50'.
- p. 187, line 10, read 'deranged' for 'not competent to calculate'.

¹ E.g. a purely abstract point of view: J. P. Serre once told me that he saw the mathematical core of the Chinese remainder theorem in a certain result of cohomology theory.

THE SCEPTIC AT BAY

It is good that Professor Ayer has written this book,¹ if only because it is a rounded treatment of a subject in one of the Wittgensteinian ways of handling philosophy. There have been at least two other works of recent times that have also provided rounded treatments of a philosophical problem in a Wittgensteinian way. There are of course differences of outlook, opinion, and method in all of these, but this does not prevent them from being different versions of Wittgensteinianism. Perhaps Professor Ayer does not regard himself as a Wittgensteinian, but this is a matter of classification, and it is legitimate, if we want to, to put together all treatments that are dominated by one of the chief Wittgensteinian trends (provided they are not discordant with other main trends). Even so, he is the least Wittgensteinian of the Wittgensteinians.

Ayer devotes a large proportion of his book to the question of scepticism and certainty. It is made plain that the opponent is always the sceptic, who is constantly challenging any philosophical statement by asking, How do you know? or How can you justify? and every attempt made to answer these challenges is in its turn met by the same challenges all over again. Ayer spends the rest of the work exemplifying this situation. He does so in detail for the problems of perception, memory, and knowledge of others. In the course of doing so, he ranges very widely over a number of other problems such as induction, the thesis of physicalism, and so on; he examines numerous sceptical arguments and counters them with great care; and his writing flows on with his usual attractive rhythm.

His way of grappling with the sceptic's challenge is this. He admits its force. He admits, in fact, its overwhelming force. He admits that there is no straightforward answer to the challenges, that, in fact, no answer that can be given directly to a given challenge is sufficient, because the sceptic is correct in repeating the challenge against each individual attempt at answering the previous one. But, having admitted this, Ayer proceeds to scrutinise the type of argument that is used, or more correctly, the type of demand that the sceptic is making. He contends that the sceptic, rightly unsatisfied with answers that are given him, is pressing for an answer that would be satisfactory, but one that can never be given. And the reason why it can never be given is that, just when the answer has reached the stage of being no longer questionable it becomes a tautology. It is therefore vacuous and cannot serve as a justification for any claim to knowledge. Now Ayer considers that the victory of the sceptic is a hollow one just because the defence against it cannot in principle be fulfilled; and he holds that, because the challenge is one that cannot in principle be met, it cannot properly be made. This thesis Ayer exemplifies in the several topics that he discusses.

¹ A. J. Ayer, *The Problem of Knowledge*, Penguin Books, Harmondsworth, 1956. Pp. 224. 3s. 6d.

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Certain questions arise out of this. . Is the conclusion correct that because the sceptic's challenge cannot be met in principle there is nothing really serious about it? Then we may wonder whether the treatment of the subject has a bearing on the philosophy of science. Again it is worth reflecting on a strange twist that this theme has taken historically. Then there is the question of the significance of the problem.

First, the historical position. Perhaps the most famous challenge of scepticism was Hume's attack on metaphysics. This reappeared in the present century in the philosophies of logical analysis, logical positivism, and language analysis. The traditional aim of the sceptical philosophies was to find a criterion by which we could relegate certain sorts of metaphysical assertion, such as those to do with the belief in God or immortality of the soul, to the limbo of things that must be totally rejected, while at the same time allowing commonsense assertions about the existence of objects, about the past, about memory, and so on, to remain and have the status that the man in the street would accord them. Various principles or forms of one principle have been put forward, both by Hume and his modern successors, mainly nowadays some principle of verifiability, which could discriminate between these two classes of assertion. But it has been found impossible to develop a principle that would carry this out, and Professor Ayer's book is, in fact, one of the strange consequences of this lack, for what was found was, that whatever criterion was proposed to rule out metaphysical assertions could, in the end, be turned on the sceptics who used it, so that it ruled out all the ordinary commonsensical assertions that they wished to make as well as those that they wished to disallow. Thus it is the sceptics who are themselves at bay, and there is no doubt an uneasy suspicion that, if any means were found of allowing back into circulation the ordinary statements that are required, there would be no way of keeping out the metaphysical statements that have been so obnoxious.

Another interesting historical light on the subject is this. According to Ayer the only answer to the sceptic would consist of a tautology, which of course would not be satisfactory. But there is clearly another possible answer. What is required is an answer that shall be both unquestionable and non-tautological. Ayer would naturally say this is impossible; but let us ask what such an answer would be like. It is easy to see that such an answer would, in fact, be what was historically known as an *a priori* synthetic proposition. I mention this, not because anyone (whether Wittgensteinian or not) would believe in the possibility of such propositions, but to show that the issue is exactly that between Hume and Kant; for Kant introduced the *a priori* synthetic proposition precisely to justify knowledge in an absolutely unquestionable way and yet without the vacuousness of a tautology. So the issue in quite simple terms is between *a priori* synthetic propositions and scepticism. One can even go further, since the sceptical attack throws

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doubt on the reliability of memory, of induction, of knowledge of objects around us, of other people. It is clear that the consequence of the sceptic's challenge, if successful, is solipsism. Hence the issue is fundamentally between *a priori* synthetic propositions and solipsism.

It is, I think, germane to bring in something of the psychological significance of the problem. Solipsism means 'I alone exist'. Now this implies at least two things. One is that what I take to be the world of memory, of objects, of other people, is entirely of my own making. It is a very omnipotent creation of mine. Secondly, it means that things that happened in the past, other people, and objects around me that are ordinarily taken to exist have been abolished by me, annihilated—a very omnipotent destruction. Thus defeat by the sceptic's argument carries with it the apprehension that I am a being of absolute power carrying out simultaneously an omnipotent act of creation and an omnipotent act of destruction. The sceptical attack was designed in the first case to abolish assertions about the existence of God. That is father killing, and the consequence was found to be that this attack killed off everything else as well and left me to create myself and all my surroundings. This way lies madness.

Perhaps philosophers who make much of this problem in their professional lives are not so deeply concerned about its personal consequences; for it is common enough to read that though solipsism is irrefutable it is unbelievable and we had better disregard it. Still, suppose we take it seriously and consider what it would be like to believe in solipsism. We could not trust our memories, so we should be unsure of the past, and if we did they would be no reliable guide to the future, in the present we should be all alone, and as regards the future we should not know what to expect at all. Anything might happen. We might fail to get anything we wanted and suffer the most frightful privation. We should live in a mood of complete uncertainty, not knowing what to do next and feel thoroughly lost and empty. A situation of more grim despair would be hard to imagine. The uncertainty and emptiness would be felt about everything, but it would be most poignant about the complete absence of any other human being. Disruption of every kind of knowledge would mean not simply intellectual ignorance but the absence of knowledge in the Biblical sense and isolation from any personal contact. Now solipsism reflects an experience that probably is or will be known to most of us, apart from states of acute melancholia or mania, only twice in life—just after birth, and when dying. There can be other moments when we find ourselves alone—even in the presence of another person, as Winnicott and Tranekjaer Rasmussen have pointed out—but most of our other losses, occurring through the death of someone, are less absolute, though sometimes not much less, in that we still have some contact, however inadequate, whether with a 'bus conductor, a colleague, or a tobacconist.

If the emptiness is to be overcome, certainty has to be found. Now when certainty is attained by means of *a priori* synthetic propositions, this means not only that we demand security about our position in the present, *vis-à-vis* other people, some hope for the future, and some reliance on the past, not only security of a reasonably reliable kind but of a kind that is absolutely certain. And this is a demand that would be made only by those who are inordinately apprehensive, in terror of feeling completely empty. (It is at this point that the existentialist philosophy of The Nothing turns out to be so closely allied to logical positivism.) Thus absolute emptiness is countered by a demand for absolute security; solipsism is countered by the *a priori* synthetic proposition.

The psychology of the position, it may be urged, may help us to free ourselves from the bondage of the problem—but the problem surely remains. There *is* a problem but I wonder if it is so complex. Let us revert to Ayer's conclusion, suggesting that the sceptic's attack is not, after all, so serious. I think that his conclusion is right—down to all but the last clause. There is no reason to conclude that the sceptic's attack can be disregarded. In my view, it finds its target completely; there is no answer at all (so far as the position attacked is concerned): no unquestionable justification for assertions about the past, objects around us, or other persons can be given—there is no certainty at all. I do not agree with the conclusion that the sceptic is demanding an unreasonable form of proof. It is the claimant to knowledge (of a certain kind) who is making an unreasonable claim when pulled up by the sceptic. The situation might be shown thus:

Claimant: The sun rose yesterday, there is the sun now, here is my brother. . . .

Sceptic: How do you know, how do you justify . . . ?

Claimant: I know because . . . , I justify on the grounds . . .

Thus the claimant displays his assumption that he can know or justify (i.e. with certainty). So in asking for the justification, which turns out to be impossible to fulfil because of having to be an *a priori* synthetic proposition (Kant) or a tautology (Ayer), the sceptic is not demanding too much—the claimant has offered too much and the sceptic is only asking whether the offer can be made good. And the claim fails. It is not the sceptic's demand for its substantiation that is at fault but the claimant's contention. If a crank sets out to square a circle with ruler and compass, it is not an empty victory for the sceptical mathematician who shows that it cannot be done. What does the difference come to? If Ayer is right and the sceptic is making an illegitimate demand, the conclusion is simply that knowledge is possible, and Ayer's method does not enable him to add anything further of a positive kind. But we know that already—we know that in some sense or other we have some knowledge. What we want to know is *how* it is possible, what kind of structure knowledge has in order to be possible. Ayer shows

that it cannot be absolute or cannot carry a guarantee. Is that all that can be said? If the sceptic's demand is fair but the claimant is overbidding his hand, we at once have the question, which cannot arise from Ayer's approach, of what sort of thing knowledge is that carries no guarantee.

To consider this, let us return to the sceptic. The sceptic is right: there is no certainty—or guaranteed knowledge—at all. And this is surely the reality. It might happen that all a person's expectations would meet with the most frightful rebuff, that no-one else should exist, and so on. The truth seems to be a simple one: this may happen or it may not happen, and there is no way whatever of knowing which.

Does this mean that there is no claim that can properly be made? Is the only alternative to solipsism the *a priori* synthetic proposition? Surely there is an alternative—to proceed without any certainty at all. And this can be done in the following way. Things may go wrong in every way, as regards our judgments about the past, our perception of things and persons in the present, and our expectations about the future. We have no justification whatever for asserting that things are as we think they are, not even for supposing that they probably are. *But they may be—we do not know that they will not.* And that is all the security we have. Moreover, for the purpose of realising our wants apart from our inner apprehension about it, that is all we objectively need.

To turn briefly to the last of our topics, the philosophy of science. To me the Wittgensteinian methods seems to have no bearing on it. Ayer himself, in the present work, disclaims any intention of doing philosophy of science. He regards his task as purely philosophical, and he makes it explicit that the results can only be tautological and trivial. There is surely something strange in divorcing the problem of knowledge from philosophy of science: scientific knowledge is after all the nearest to being trustworthy that we have (and closely connected with it the historical knowledge of antecedents of change); no scientific knowledge is absolute or certain; in philosophy of science the problem of knowledge is the problem of testing unguaranteeable hypotheses; how then can we reasonably raise the purely philosophical form of the question, to do with guaranteeable knowledge? In the philosophy of science there would be an air of unreality about this question, and a different one would take its place. Hence Ayer would seem to be making an abstraction from scientific knowledge; and, if so, it is hardly surprising that he regards philosophical conclusions as inevitably trivial. Despite my opinion, however, of the lack of bearing of this form of philosophising on the philosophy of science, I have considered it appropriate to review this work at some length in this *Journal* because philosophers of science will wish to estimate the position of the most influential current mode of philosophising.

J. O. WISDOM

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La Structure algébrique des théories mathématiques. By Léon Henkin.
Gauthiers-Villars, Paris, and E. Nauwelaerts, Louvain, 1956. Pp. 52.

HERE we have an important and stimulating book which should be read by mathematicians, logicians, and by those scientists and philosophers who realise the complementary rôles of language and algebra in forming the skeletal structure of any scientific theory.

Everybody is familiar with the significance of the group-theoretic viewpoint for the attainment of universally valid laws. One need only mention the invariant theory in geometry, the group of Lorentz transformations in special relativity, the crystallographic groups, and the group-theoretic approach to spin and valency. If we ask why algebra in particular contributes to the understanding of such diverse theories, the answer is simply that it enables us to abstract the essential patterns underlying them and to arrive at meaningful generalisations. On the other hand, we too often neglect the more subtle aspects of language and logical presuppositions in the formulation of our theories.

In the book under review the author discusses with great lucidity the problem of constructing a faithful algebraic model of a class of mathematical theories. He uses Stone's theorem on Boolean algebras to stress the importance of one question: is a given model faithful in the sense that all laws, valid in the systems represented, apply also to the model? Clearly this is belied by the very use of the word 'model'. He then deals with the rôle of language in a mathematical theory and emphasises the inadequacy of the Boolean system. This chapter makes easy reading because formal definitions are relegated to an appendix, which contains also a new proof of the completeness of the language *via* the corresponding theorem for Boolean algebra. However, the main goal of the book is reached in Henkin's treatment of the cylindric algebras of Tarski and Thompson. The author presents his own example of a non-representable algebra and concludes with some conjectures as to the future development of this new type of algebra.

There is a bibliography which the author has wisely and carefully restricted to make it of maximal service to the uninitiated. The monograph is produced by the Tyme-Offset process. The list of errata would be very long; for instance, the symbol of set exponentiation is omitted almost as often as it is inserted. But, fortunately, Henkin's clear and cautiously graded exposition renders most of these typographical blunders trivial.

The author and the publishers ought to be congratulated on having produced a book which provides an excellent introduction to modern researches in a field from which many are turned away by reason of the

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cold austerity of algebraic formalism. It is to be hoped that the monograph will serve the dual purpose of attracting young mathematicians and philosophers of science, and of impressing the relevancy of language in mathematical theory upon those who still consider its study somewhat (*sit venia verbo*)—‘Carrollian’, in the lighter sense of the term.

WOLFGANG YOURGRAU

Science and Human Life: Successes and Limitations. By J. A. V. Butler.
Pergamon Press, London. Pp. 176. 15s.

THE author of this book, J. A. V. Butler, D.Sc., F.R.S., is Professor of Physical Chemistry in the University of London, at the Institute of Cancer Research (Royal Cancer Hospital).

The aim of the book is to examine the question, ‘Can human conduct be based exclusively on scientifically ascertained knowledge?’ The answer which the author gives to this question is ‘No’. He considers that human beings require beliefs which cannot be rationally justified nor receive immediate empirical justification: I say ‘immediate’ because I think the author would contend that the beliefs of which he is speaking, which are of the nature of myths, could be considered justified if there were reason to consider that they had been potent factors in holding a society together for some considerable stretch of time, as, for example, for several centuries: they would, in anthropological terms, be seen to be ‘functionally justified’. He seems clearly of the opinion that contemporary Western society suffers greatly as a consequence of its lack of myths but he gives us no indication of how a modern society deprived of myths could set about acquiring new ones. To expect this would, however, be to ask more than the author promises, for he only intends, I think, to set the limits of what we may expect from empirical science, and to give some indication of what science has so far achieved in its study of man. This is already a sufficiently ambitious programme, and the book ranges over such topics as the method of science, the present achievements of biochemistry, of biology, and of anthropology; there are discussions of our present knowledge of the human brain, and of the relations of brain and mind.

It is clear that such a scope is incompatible with precise scholarship, though not necessarily with interesting contributions to philosophy. Dr Butler, however, does not pretend to be *très calé en philosophie*, and the book is clearly intended for the general reader, and should be judged accordingly.

Judged, then, from the point of view which is appropriate, the book seems to me to be admirable. Too often the general public is introduced to a ‘science’ which is on the eve of solving, if not all human problems, at

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least some of the major problems which have puzzled legislators, moralists, or philosophers, for millennia.

Dr Butler has a firm grasp of the limitations of scientific method in general, and he fully appreciates and clearly explains to the layman how little progress has in fact been made with some of the problems which are in principle amenable to scientific investigation, such as the nature of the mechanism of the brain, or the manner in which genes produce their overt effects.

Personally I have found it intensely interesting to see the way in which a highly competent scientist, who is living, not in the penumbra of the last, but in the light of the present century, struggles to construct some tolerably adequate picture of human nature in the round.

And if Dr Butler's man bears a striking resemblance to Aristotle's, that is something about which I should be the last to complain.

J. S. WILKIE

La Mécanique au XVIII^{ème} siècle (des antécédents scolastiques à la pensée classique).

By René Dugas.

Dunod Editeur, Paris, 1954. Pp. 620. 3,900 Frs.

IN this book Dr Dugas, whose magisterial *History of Mechanics* recently made a welcome appearance in an English translation, provides us with a valuable survey, from a somewhat different standpoint, of the mechanical ideas of the great thinkers of the seventeenth century. Unlike his general history, the present book not only concentrates primarily on the underlying philosophical ideas of the subject but is more in the nature of an anthology. It contains much invaluable source material which has not previously been easily obtainable.

The plan of the book is as follows: first a concise survey of previous ideas from Aristotle and Archimedes through the medieval period up to Bruno and Tycho Brahe; then two short chapters on Kepler and Stevinus of Bruges, followed by longer chapters on Galileo, Mersenne, and Gassendi; after this a magnificent chapter of over sixty pages on Descartes, a somewhat surprisingly longish chapter on Pascal and another chapter on Cartesian physics (as distinct from the physics of Descartes); then we come to the three outstanding figures of the second half of the century Huygens, Newton, and Leibniz, to each of whom a long chapter is devoted; then a chapter on the theory of external ballistics and the resistance of fluids from Tartaglia and Galileo to John Bernoulli; finally four general chapters on the interactions and mutual criticisms of the three main schools of thought at the end of the century, Cartesian, Newtonian, and Leibnizian.

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Perhaps the most striking feature of the book is the way in which, by means of carefully selected passages from his writings, including his correspondence, the development of Descartes' ideas on mechanics is presented. Although it must be admitted that, broadly speaking, the judgment of Whewell still holds good, namely that of the fundamental principles of mechanics which were easily obtainable at the beginning of the seventeenth century Descartes grasped as few as was possible for a man of genius, nevertheless one's respect is enhanced for his great pioneer attempt to create a theoretical physics based solely on mechanical laws.

It is a pity that Dr Dugas has confined his attention too exclusively to the general history of ideas, thereby producing a somewhat lop-sided picture. This failure is perhaps excusable in France where there is not such a rigid division as here between science and philosophy, for the French readers of Dugas' book will presumably have some understanding of the technicalities of classical mechanics. But in this country the separation of philosophy of science from science itself is of long standing and any book which, however unintentionally, encourages philosophers to think that they can dismiss the essence of scientific investigation as mere technique and concentrate solely on the 'ideas' cannot be regarded without some regret. Nevertheless, when this has been said, it must be admitted that Dugas has performed an inestimable service for those students who, knowing what mechanics is about, are anxious to understand the tortuous process of how it actually came to be developed.

G. J. WHITROW

Une Tentative d'interprétation causale et non linéaire de la mécanique ondulatoire (la théorie de la double solution). By Louis de Broglie.

Gauthier-Villars, Paris, 1956. Pp. vii+297. 3500 fr.

A **WRAK** argument does not become stronger by repetition ; nor does it gain in effectiveness by dilution. Professor L. de Broglie, in this new book, has expanded to nearly 300 pages the story, which he has already told us many times in shorter essays and articles, of his pioneering efforts to develop a 'wave mechanics' of atomic phenomena on a classical basis, of his (as we now learn) reluctant adoption of the conceptions of quantum theory, and of his recent return to his old ideas.

The book brings physicists no element which could possibly induce them to alter their considered judgment, passed thirty years ago, about the impossibility of a description of quantal processes in classical terms ; in fact, it has the singular distinction for a physical treatise of not containing a single piece of new experimental evidence which could conceivably bear upon the

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problems discussed at such length. The theory of the 'double solution' suffers from a lack of uniqueness in the determination of the non-linear terms of the wave-equation, which puts it on a par with the aether models of the last century.

One would like to say in favour of this book that it assembles in a convenient form all the elements of this 'Scheinproblem', which will have a place of honour in any future 'Budget of Paradoxes'. But Professor de Broglie promises us another book; the theme obviously allows of infinite variations.

L. ROSENFELD

An Introduction to Cybernetics. By W. Ross Ashby.

Chapman & Hall, London, 1956. Pp. ix + 295. Price 36s.

'CYBERNETICS' is a term coined (or rather revived) by Norbert Wiener. His book of this title, which appeared in 1948 was a scientific best-seller of which very few people have read more than the introduction. Wiener's great scientific contribution to the subject was an ingenious mathematical theory of *purposeful* operations on stochastic series. Curiously enough, the major, mathematical part of his book (which very few people have read) had far less influence on contemporary thought than his introduction, which almost everybody has read. Whatever Wiener's intentions may have been, the main effect of his book and of his many lectures in all parts of the world consisted in drawing the attention of biologists, psychologists, and of representatives of other essentially non-mathematical or semi-quantitative sciences to the theory of feedback control systems, which was developed by engineers in the early thirties. (Hazen and Nyquist may be considered as its most important pioneers.) It so happened that soon a wealth of other new ideas started flowing through the propagandistic breach opened by Wiener from the quantitative into the semi-quantitative sciences. Most of these came from Shannon's Statistical Communication Theory, but the general aspects of computer and automaton theory and of operational research soon followed, and in the past years a somewhat heterogeneous compound of all these constituents is sailing under the flag of Cybernetics, at least on the Continent of Europe. (The term is strongly disliked and carefully avoided in British and American 'quantitative' circles.)

It is interesting to see how these general ideas reflect themselves in Dr Ashby's highly original mind. Wiener defined Cybernetics as 'the science of control and communication in the animal and in the machine'. This is general enough (too general for many people's liking), but Ashby makes it even more general by making Cybernetics the science 'not of things but

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of ways of behaving'. In other words it is the functional science of systems in the abstract. Throughout the book the author has done his best to teach his fellow-biologists, to whom this book is mainly addressed, the art of thinking in abstract terms, with the emphasis on function, not on structure. I am unable to make sense of his other definition (on p. 4), that 'Cybernetics might, in fact, be defined as *the study of systems that are open to energy but closed to information and control*—systems that are "information tight"'. Nor can I see that he has taken this definition very seriously.

What the author has done in fact was to give an extensive *dictionary* of terms and concepts which have originated in the quantitative sciences to the non-mathematical scientists. He has not restricted himself to the new sciences (cybernetics and communication theory), but has included the general ideas of classical dynamics, set theory, topology and probability theory, though he has carefully avoided symbolic logic. He has taken great care that his readers shall be 'doers of the word and not hearers only, thus deceiving their own selves' by giving them for every new concept a number of carefully thought out, often ingenious, exercises to solve. This is somewhat unusual in a book addressed to 'workers in physiology, psychology and sociology', and one wonders how successful the author will be with his excellent intentions. It is true that almost any intelligent schoolboy could solve his problems without a great effort, but the adult scientific worker is seldom of the type which delights in solving brain-teasers. It is more likely that most readers will be satisfied with acquiring the vocabulary by reading the text, but this in itself may well be a great benefit to some. An idea of this vocabulary may be gained by the following, somewhat random selection of section headings. 'Transformations, vectors, coupling, feedback, stability, the black box, isomorphism, constraint, the Markov chain, quantity of selection, amplifiers.' One of these, the concept of stability by feedback is probably the greatest gift of engineering to the semi-quantitative sciences. By the work of Tustin and others it is penetrating economics, where its quantitative mastery may well result in progress comparable to that which its intuitive understanding by Keynes and Beveridge has brought about since the dismal thirties. If only somebody with sufficient literary gifts could insinuate it into the heads of politicians!

The reviewer is somewhat doubtful whether Dr Ashby's highly praiseworthy effort will be didactically successful. This book is an introduction to elementary abstract thinking, with only occasional illumination by models. Few people are capable of sustained abstract thought, and these are usually mathematically gifted, there are many more who prefer thinking in terms of models and analogues. Having read this book by the author as well as his previous *Design for a Brain* the reviewer suspects that Dr Ashby himself belongs more naturally to the second type than to the first. The few places where models are introduced, such as the power amplifier on

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page 239, stand out as the purple patches of the book. It might have been better to have more models and less exercises. It is also regrettable that the author does no more than whet the appetite of the reader on the last page with a brief mention of 'intellectual power amplifiers'. If this is kept for a later book, let us look forward to it!

D. GABOR

General Systems: Yearbook of the Society for the Advancement of General Systems Theory, Vol. I 1956. Edited by L. von Bertalanffy and A. Rapaport. Pp. 162. (No publisher or price. Obtainable from the Editors at U.C.L.A., Los Angeles, and University of Michigan, Ann Arbor, respectively.)

SINCE 1930 many have considered that the time is ripe for a morphology of knowledge, covering anything from the fundamentals of physics (Eddington) to all space-time processes (various groups, particularly in the United States, including the above Society). This tendency is now so widespread and includes so many distinguished names that there can be no doubt that it is an expression of the present situation of exact science, implying great potentialities and also considerable dangers.

In view of the pressing need for the unification and clarification of fundamentals the potentialities of this situation require no emphasis, and it is therefore the dangers which call for analysis. Most of the participants in this movement appear in varying degrees to exaggerate their own achievements and to indulge in premature and somewhat indiscriminating formalisation and generalisation. These features render an objective valuation of their work difficult; for example the passage of ten years has been necessary for a widely acceptable critique of Eddington's *Fundamental Theory*. The danger here is not merely that adherents may exaggerate what has been accomplished but that opponents may neglect constructive achievements hidden in a mass of formal analysis. And it may take a long time for both sides to discover their errors.

This dual danger is illustrated in the present volume. The valid aim behind the Society's programmes is the desire to prepare the way for unification. But the present volume reveals that touch of exaggeration which may repel those who might benefit from studying some of its contents.

This yearbook is not a product of the Society's activities, but a collection of fifteen essays, eleven of which have already been published elsewhere up to seven years ago, at least six of the authors being distinguished specialists in various branches of biology. The essays range from Bertalanffy's exposition of his conception of General Systems Theory to Krech on 'Dynamic Systems

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as open neurological systems'. To take one example, it is valuable to learn what M. Calvin has to say on 'Behaviour: Imbalance in a Network of Chemical Transformations'.

The element of exaggeration is occasionally unmistakable. Thus Boulding says: 'General Systems Theory is the skeleton of science, in the sense that it aims to provide a framework. . . .' But a valid 'General Systems Theory' does not yet exist. Moreover those papers which concern themselves with this long-term aim display in some degree one or more of the following failings: undue stress on similarities with relative neglect of differences between one realm and another, premature generalisation, exaggeration of the novel content of the conclusions put forward, or tendency to evade the difficult but crucial task of a critique of the data and of established theories. These weaknesses would not matter so much were the aims less far-reaching, but as it is the average reader may find those passages most satisfying which deal with specific empirical data. This will not be felt when the Newton of a comprehensive theory of complex processes publishes the authentic formulations which so many are now seeking.

But anyone who is genuinely interested in the problems of a unified theory of complex phenomena (in any realm, however narrow or wide) should not neglect those essays which are relevant to his subject matter. Bertalanffy's emphasis on the importance of open systems in biology involved no new idea, but is important, for it leads to still unresolved problems, such as the scope of the different entropy concepts in relation to living systems. If the reader thinks he can discover flaws in some of the essays in this volume, these may be no worse than the often neglected flaws in established theories. Since all scientific theories (with the exception of the theory of the two hundred and thirty crystal classes) are probably imperfect, any fresh attack, such as the one presented here, is to be welcomed.

Unfortunately no attempt is made in this volume to compare and contrast the approach of 'General Systems Theory' with those of various other groups of similar aim. But a fully objective and satisfactory critical assessment of the various potentially universal methods (systems theory, information theory, stochastic theory, behaviour theory, etc.) may only be possible after either physical or biological theory has achieved another fundamental advance. In the meantime all attempts to open up new methods are useful even when they fail, and this reviewer will not condemn any such attempt which is still in an early stage. In this situation each must discover for himself what can help him with his own problems.

L. L. WHYTE

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The Basic Concepts of Mathematics. By Karl Menger.

The Bookstore, Illinois Institute of Technology, Chicago 16, 1957.
Pp. vii + 93.

This book can be warmly recommended to all who teach or learn elementary mathematics. Its aims are well set out in the author's Preface:

Algebra, analytic geometry, and large parts of the calculus, as taught today, are products of the 17th century. The fundamental ideas of those branches of mathematics are among the great legacies of that period and have ever since belonged to the most precious heritage of mankind. But with those ideas, the 20th century has also inherited the form in which they were presented by their discoverers. It has inherited the noun *variable* in several meanings that are completely at variance with each other, the noun *constant* in uses that are blatantly self-contradictory; and terms such as *parameter* and *indeterminate* that are utterly obscure. It has further inherited the indiscriminate use of the letter x in more than ten altogether discrepant types of procedure—Renaissance mathematicians practised some kind of x -olatry. Because all those terms and the symbol x have not been, and are not being, used consistently, their vocabulary and grammar have never been written. This is why those crucial chapters of the theory of the mathematical language have not been, and are not being, taught and why, in the process of learning mathematics, important meanings and rules must be surmised. The deepest difficulties of mathematical education thus are not due to shortcomings in education. They are due to procedures in mathematics.

Although the book is primarily addressed to beginners, it is anticipated that it will also interest the accomplished mathematician and scientist; they will at least be interested in the way the author overcomes the difficulties mentioned above. He himself appeals 'to everyone desirous to clarify his ideas about some of the basic concepts and fundamental procedures of mathematics'.

J. H. WOODGER

Calculus: A Modern Approach. Second, enlarged edition. By Karl Menger.
Illinois Institute of Technology, 1955. Pp. xxiv + 303.

ALTHOUGH the principal object of this book is to bring about a revolution in the *teaching* of the calculus, its interest and significance is philosophical as well as pedagogic because of the author's discussion of the ideas of function and limit.

The author defines the word 'function' and not the usual phrase ' y is a function of x ' which raises the question of the nature of x and y . He defines a function as a class of pairs of real numbers in which two pairs

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which have the same first element have also the same second element. Apart from certain differences in notation from the traditional method, notably dropping brackets and writing fx for $f(x)$, this definition makes it possible to distinguish clearly between the function f itself, which is a class of pairs of numbers, and the value of the function fx at x , which is a number.

The author's new formal definition of a limit is : L is the limit of f at a if fx is arbitrarily close to L for all numbers x sufficiently close to a . This replaces the current terminology 'the limit of $f(x)$ as x tends to a ' which in its turn replaced the phrase 'the limit of $f(x)$ when $x = a$ '. The latter tended to obliterate the important distinction between the limit and the value of the function at a , but the former suggested the idea of 'motion' which is now regarded as irrelevant.

There is much else in the book which challenges the customary notation and exposition of the calculus, both differential and integral, but discussion of these features hardly falls within the purview of this *Journal*. There is no doubt that this is a book of the greatest pedagogic importance. Owing to the marked divergence between the author's methods and those which are now current, it is unlikely that his will be generally followed, at least in the immediate future. Nevertheless, this is a book which should be read by all who teach the subject for it will give them fresh ideas and inspiration.

G. J. WHITROW

The Image. By K. Boulding.

The University of Michigan Press, Ann Arbor, 1956. Pp. 175. 30s.

IN this stimulating and ambitious essay, dictated, as he tells us, in a mood of 'intellectual exaltation', Professor Boulding discusses his concept of the message-image relationship. He shows that the concept of the image may be useful and illuminating in many enquiries, and suggests that the theory of the image, or the theory of knowledge, should be the subject matter of a new science of 'Eiconics', or, at least, an important unifying principle in the sciences. The message-image relationship, as he describes it, is indeed important: whenever an organisation, a human being, an organism, or even a mechanical control system, reacts in response to the stimulus of a 'message', it follows, in Professor Boulding's argument, that the message has changed an 'image', and that the new behaviour is the response of its owner to the changed image. Professor Boulding's subject is the image, its organisation and nature, its growth and change, and its control over behaviour. He discusses the image in organisms as well as in individuals, and devotes several chapters to the rôle of the image in human society.

The personal image is the most complicated. The individual is self-conscious, aware of himself and of his own image. His image includes

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his location in time and space, a picture of himself and of the universe, of physical relations, and of his relations with other people. It includes his complicated array of emotions and values. Pictures in the image are held with different degrees of certainty. Parts of the image are only in the subconscious. Parts are shared by other people, and are 'public images'. The many parts of the image are acquired in innumerable ways. They are built up from personal experience, from family and formal education. Besides getting information from his senses, man can transmit and receive 'symbolic messages'. He is continually receiving messages, filtering them through his value system, and adding them to his image. His image includes a picture of society, and of his own position in it. The structure and history of society, and the images of individuals, are interdependent. The individual's image of the world, for example, may depend on whether his school atlas was prepared by a German nationalist or by an English imperialist. The nature, growth, and activities of organisations depend on the images of the people who belong to them, and in turn influence the images of both members and onlookers.

How much attention is paid to 'eiconics'? Professor Boulding discusses new work in the natural sciences, information theory, and cybernetics. He suggests that psychology and anthropology are largely 'about images', whereas economics is mechanistic. History is concerned with images (but, curiously enough, historiography is omitted from the list of 'eiconical' studies). This interest in the image is the common ground between the disciplines. How the study of the image is to be a new science, however, is a little obscure. That historiographers, control engineers, psychoanalysts, and geneticists should be aware of their common interests may be a good thing. How a new discipline is to be made of their community of interest is harder to see, while the proposition that it should be depends, of course, on Professor Boulding's view of the rôle of the image.

It is very easy to agree with a good deal of what Professor Boulding says. It is easy, for example, to see as he does the relationship between the image and the disciplines of psychology and anthropology. At times, however, the argument appears to depend on particular uses of the word knowledge. When he says that the carbon atom 'knows how' to unite with four hydrogen atoms or two oxygen atoms, he also says that this is only 'a figure of speech'. The same word, image, is employed in the discussion of the behaviour of the amoeba and the behaviour of the President of the United States. Professor Boulding does not say 'The clock tells the time, therefore the clock must know the time'; but his argument is sometimes rather like that.

The wide scope of this entertaining essay offers Professor Boulding the opportunity for many generalisations. Many of them are shrewd and illuminating, and some, like his comments on the culture of the universities,

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are witty as well. Others, like his observation that poor societies are apt to be more materialist, and richer societies more concerned with 'spiritual affairs', might have been revised. And it is a pity that he sometimes allows himself such expressions as 'soon-to-be-frustrated inside-dopesters'. But the essay should be enjoyed by anyone who is interested in the theory of knowledge and stimulated by speculation.

G. C. ARCHIBALD

Perception and the Representative Design of Psychological Experiments. By Egon Brunswik.
University of California Press, & Cambridge University Press, London,
1956. Pp. vii-xii + 154. 37s. 6d.

BRUNSWIK did not live to see this volume published. This is not an obituary, yet the book I am reviewing summarises the work of this great psychologist.

There is little that has not appeared before, the book being in many ways a re-editing of earlier works, but it is the fullest exposition of his thoughts that there is. Brunswik does not make easy reading, at least not until one has committed to memory and grasped the significance of quite an array of terms. I fear this difficult style (Brunswik originally wrote in German) will be a barrier to the spread of ideas contained in the book which are worthy of the attention of psychologists and philosophers alike.

To give a very simplified account of his position, he made a plea for a type of experimental design in psychology which is more representative, and gives a better sampling of the full richness of ordinary life situations. For this purpose he said we need a 'multivariate' design where, instead of standardising all but one variable as in the classical work in psychophysics and other areas, all the relevant variables are allowed to vary in a random (though measured) way or in a way which 'samples' the actual true life situations. Not only must the stimuli be varied as recommended by R. A. Fisher but also the subjects in the experiment and also, if appropriate, the experimenters. By this means only will the results have any degree of generality.

The other corner-stone of his theory is his emphasis on what he called the functionalistic achievement of the organism in its perception. By this he meant the degree of orderliness and consistency with which it can cope with an environment which is potentially in a state of chaos. Such stability is achieved by what he called the distal focusing of stimulus complexes into 'constancies'. These are the well known constancies of experimental psychology. Brunswik distinguished between the 'proximal' stimuli such as the retinal shape and size of an object and 'distal' stimuli such as

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object constancies. The latter are constructed by a 'probabalistic weighting' of the former and are the stable end products of perception, gained largely through experience. This implied duality of stimulus classes is perhaps unfortunate and unnecessary, it calls to mind the familiar distinction between sense data and objects, and J. J. Gibson's 'retinal' and 'visual' worlds. All that is required is to say that object constancy is the result of a 'confluxion' of stimulus complexes, the constancies themselves do not have independent identity as stimuli. Such a view is consistent with Brunswik's later formulation and he did in fact talk of such 'confluxion' or 'focusing' of stimuli.

His exposition of the rôle of attitude in psychological experiments in chapter XIV should be read by all who are interested in perception. Lack of clarity in the definition of the task asked of the subject in the experimental situation has caused widespread confusion in the literature. Brunswik's writings on this matter are most refreshing and helpful.

In the second part of the book Brunswik turned from a basic exposition of his theoretical position to review experimental work which he considered 'representative'. Some of this work, especially the experiments on weight illusions, I find in no way different to classical experiments. The experiment on the judgment of faces, while admittedly representative, seems trivial and uninspired to me. Many well known experiments and procedures which are not mentioned are, as I see it, well qualified to be labelled both representative and functionally orientated. I am thinking of time sampling studies of child play, Piaget's study of children's thought processes and sensori-motor habits, the War Office selection procedures for officers during the war, and much of modern sociology and anthropology. Such methods are used where appropriate, and without fuss. Brunswik's critical remarks are, however, probably warranted within the academic study of perception and learning, where there has been too often a narrow conception of experimental design. Here Brunswik's ideas are like a breath of fresh air.

It would be a mistake to suppose that the older methods are now useless and superseded by representative design throughout the whole domain of psychology. Brunswik did not claim this, indeed he stressed that for certain problems the classical, univariate, controlled experiment is appropriate, as long as the legitimate limits of generality of the results are not exceeded. An overdue emphasis on representativeness may have its own dangers, for pushed to its logical conclusion the ideal experiment would be everyone's and anyone's observations on everyday life, but even that may be a good antidote for learning theorists emerging from a generation of pre-occupation with rats in mazes, and dogmatic assertions about the rôle of the hypothetico-deductive method in psychology. We owe a lot to the late Egon Brunswik.

I. P. HOWARD

REVIEWS

Problems of Consciousness: Transactions of the Fifth Conference, 1954.

Edited by Harold A. Abramson, M.D.

Josiah Macy, Jr. Foundation, New York, 1955. Pp. 180 \$3.50.

THIS is another of the volumes, by now becoming well known, in which a number of eminent contributors open the ball with papers and have their subsequent discussions taken down in shorthand. Some of the volumes produced in various fields have been remarkably good considering their impromptu nature. The conferences were obviously worth holding and the volumes are of some value as a stimulus.

But it cannot be denied that some of the volumes have grave defects. In a previous review in this *Journal* on previous transactions about consciousness I drew attention to the fact that the participants had no definite problem before them. The present volume, the last of the series, says that they have discovered this. The subject peters out without their ever being able to find a topic and pull the threads together. It is perhaps the weakest of all the volumes.

On page 93 we read, 'I do not know what we are arguing about.' This was the first sign of good sense.

On page 131 we find the extremely illuminating information that a correlation has been found between birth with long labour and failure to keep appointments.

On page 134 we are reminded of the need expressed at the previous conference to have a methodologist. This is just conceivably an understatement.

On page 147 an attempt is made to define a science: 'that which lends predictability to phenomena is scientific.' One is not impressed to find pronouncements like this still being made in serious discussions by first class *savants*.

On page 160 in the closing remarks we find a penetrating estimate of the position reached: 'although I still do not know what anybody else means when he says "consciousness", I have a much better understanding as to what I mean by it, even though I cannot put it into words.' In short the conference can hardly be said to have got very far; and it is indeed doubtful whether the present volume can truly be said to be about anything.

J. O. WISDOM

ABSTRACTS

Philosophy of Science, 1957, 24, No. 1

W. P. Alston, 'Is a sense Datum Really Necessary?'

It has been claimed that certain perceptual situations (e.g. complete hallucinations) can only be described in terms of sense-data. But careful survey of ordinary language reveals a large number of expressions, e.g. 'It appears that—', 'It seems that—', 'It was as if—', etc., which contain no phrases referring to sense-data, and which can be used for such descriptions. Such sentences are sometimes used to indicate degrees of probability, but they also have the phenomenological use. Moreover such sentences bring out the similarity between normal perceptions and hallucinations at least as well as sense-data sentences.

Philosophy of Science, 1957, 24, No. 2

C. Frankel, 'Explanation and Interpretation in History'

This paper analyses certain key issues in the logic of historical explanation with a view to determining the relation of 'interpretation' to 'explanation' in history. It is believed that historical explanation, although it has certain distinctive features and is frequently dependent on views about the 'meaning' or 'value' of given events, is nevertheless continuous with the logic of disciplined inquiry in other domains. An analysis of types of historical interpretation is offered in an attempt to show that arguments about the proper interpretation of historical sequences are usually genuine arguments rather than pseudo-arguments, and that they are at least in principle capable of being resolved by appeal to objective considerations.

Philosophy of Science, 1957, 24, No. 3

H. Hay, 'Free-will and Possibilities'

The paper discusses whether the believer in free-will had hold of any points omitted by determinists but not inconsistent with determinism. An examination of William James's 'Dilemma of Determinism' showed that he rejected determinism because it denied the existence of alternative possibilities and because without these regret would be unreasonable. However, James's attack was against what can only be regarded as one species of determinism, a monistic holism. Determinism in the broad sense allows us to define what is meant by the existence of possibilities. Possibility is a relative term. A possibility exists with reference to a specific situation and is what the description of that situation together with true generalisations implies when certain additional particular facts outside the situation are added. Hence, with reference to a given situation there are alternative possible outcomes, with reference to different additional particular facts.

H. Hiz, 'Types and environments'

The notion of type (like style in art, period of history, phoneme) can be analysed by environments. For this purpose one may take a restricted concatenation that

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allows one to juxtapose only such segments that actually occur one after the other. Two segments may belong to the same type if their environments are always different.

H. A. Patin, 'Pragmatism, Intuitionism, and Formalism'

This paper deals with a quandary which engaged Peirce as to the result of applying his pragmatic maxim to the concept of 'incommensurable'. Peirce's views are examined and it is then shown that later pragmatists, especially Dewey, by profoundly enlarging the notion of 'practical bearings' are liable to give a satisfactory account of abstract concepts and systems which is clearly in the spirit of Peirce's original intent, and in accord with his conception of mathematics. The solution that Peirce summarily discarded and the one that he finally offered are seen to be closely related to the mathematical schools of 'formalism' and 'intuitionism' respectively. The thesis is then advanced that although the latter has an approach strikingly similar to Peirce's last 'operational' formulation, it is actually 'formalism' whose intent and method lie within the present scope of pragmatism.

R. R. Delgado 'A possible Model For Ideas'

The article presents 'the Idea' as the differentiating characteristic between man and animal. Ideas constitute the basic tool of man. Comprehension of the structure of ideas would be useful to understand the function of different and often contradictory ideologies. This article considers the element of ideas to be: (1) Perceptible worlds, specific, individual and social. The social perceptible world is divided into linguistic, objective and professional subspheres. (2) The intuitive world, in which is described a scale of growing intuitive abstraction, differentiated into mystic and aesthetic experiences. (3) The conceptual world, hypothetically evolved through four main stages: perceptive, intuitive, conceptual and experimental. In the experimental class appear the actual forms of relativised, synthetic and integrated concepts. A division between closed and open concepts is offered, the open concept being dynamic, without absolute borderlines, forming a structural continuum and requiring for its treatment a new kind of Logic. (4) The affective field, which is reduced to a minimum by logical symbolism and mathematical forms of expression. These four intellectual mechanisms or processes are in an 'idea of the idea', an operational symbol that perhaps may be used, from a unified point of view, in psychology, logic, gnoseology and philosophy. A graphic model of the idea is proposed, and a possible classification of its individual and social types.

L. Reiser, 'Matter, Anti-matter, and Cosmic Symmetry'

The present article accepts a postulated cosmic symmetry, but holds that 'anti-matter' requires a complementarity between 'particles' and archetypal (organising) forms in the Cosmic Field of Energy. The existence of anti-matter (e.g. neutrons and anti-neutrons) no more requires the existence of 'anti-galaxies' . . . wrongly supposed to be receding from each other since the 'big bang' five billion years ago . . . than the existence of organisms requires the existence of anti-organisms.

The writer's *cyclic-creative cosmology*, based on an *energy matter* reciprocity and the continuous creation of hydrogen atoms, builds on the hypothesis that positive and negative pair-particles (electrons and positrons, protons and anti-protons) may represent

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appositely modified spiraloids in the Cosmic Field, due to differences in rotations associated with operators capable of double rotations in the hyperdimensional Field, thus creating enantiomorphic (mirror-image) forms. The relation of this conception of matter as spiraloids of energy to the recently announced Yang-Lee dissymmetry of spin-particles ('fall of parity') still remains to be explored.

Philosophy of Science, 1957, 24, No. 4

H. Marganau and R. A. Mould, 'Relativity: An Epistemological Appraisal'

This paper is the forerunner of an extensive logical analysis of the relativity idea, in which an axiomatic structure based upon the principles of topology is developed. It is meant to expose the manner in which relativity stretches from the pole of pure conception to that of factual observation, from the *a priori* to the *a posteriori*. In connection with special relativity, it is shown precisely which elements are postulational and which are verifiable empirically. The authors attempt to show how misguided it is to 'derive' relativity from such experimental facts as the Michelson-Morley experiment—as so many textbooks profess to do.

A. Landé, 'Non-Quantal Foundations of Quantum Theory'

The article aims at a better understanding of the quantum theory by reducing its principles and mathematical rules to elementary postulates of an almost common-sense non-quantal character. In particular, the strange laws of the probability amplitude Ψ as well as the periodic relation between energy and time, momentum and position are developed as consequence of simple ground postulates: continuity of cause-effect relations (implying necessarily the existence of acausal discontinuous events), reproducibility of a measurement, the postulate that there be a general structural metric of the probabilities at all, and constant statistical density in position-momentum space (taken from classical statistical mechanics). Combination of these elementary postulates leads to Planck's and de Broglie's equations and all the rest. In contrast to the current opinion, propagated by the Copenhagen School, that the 'quantum principles' of complementarity, etc., represent the ultimate bottom of theoretical analysis, the article shows that there is a more elementary background upon which the quantum theory including its physical 'principles' and mathematical prescriptions can be erected.

E. Grunberg, 'Notes on the Verifiability of Economic Laws'

The weakness of the explanatory powers of economics is due to the concern of economics with unique events where this term is taken in the sense of: all that goes on in a specified spacetime region. The explanation of unique events is always incomplete and has therefore been called an explanation-sketch by C. G. Hempel. Since such a sketch refers explicitly to a non-repetitive event it cannot be verified in the customary way by prediction. Therefore general laws must be verified independently from their use in explanation-sketches. General economic laws cannot be disconfirmed by incorrect prediction since a discrepancy between predicted and observed event may always be explained by the nonfulfilment of the *ceteris paribus* condition.

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If a law cannot be directly or indirectly verified, it may be possible to deduce it validly from true premisses. This apparently is the procedure followed in the deduction of economic laws from the rational principle. The trouble with this is that the rational principle itself can be verified only by verifying the theorems deduced from it. Thus the justification of criticism directed against the rational principle lies not in the fact that human beings do not always act rationally, but in the impossibility of strictly disconfirming the theorems deduced from it.

Since economics deals with certain aspects of human behaviour, it seems promising to deduce economic laws in which no behavioural terms appear in reference to the *explicandum* from laws referring explicitly to regularities in human behaviour.

While the suggested reduction permits satisfaction of the requirement that all laws entering explanation-sketches must be verified independently from such use, it leaves intact the difficulties arising from the incompleteness of the explanation-sketch. This difficulty alone, however, is not insuperable.

J. P. McKinney, 'Knowledge and Experience (Comment on a paper by L. von Bertalanffy: "The Relativity of Categories")'

The 'relativist' view of knowledge is an expression of the modern rejection of the 'classical' view that knowledge is an objective report on an 'absolute' world. The relativists (von Bertalanffy *et al.*) see knowledge as necessarily bearing the stigma of the experiential (biological and cultural) conditions of which it is an expression. The individual experiences in terms of the established body of categories, or what may be called the common world-picture. It is pointed out that the relativist standpoint, as an outcome and expression of the tracing of the structure of knowledge back to its basis in experience, conflicts with the view that an 'absolute' basis of knowledge is being revealed by the elimination of all elements of experience from the structure of knowledge. On the contrary, the implication of the relativist standpoint appears to be that, if an absolute core is being revealed by the reduction of knowledge to its basis in experience, *then* this absolute would have to be sought in experience itself. The view (Whorf) that the relativist standpoint should lead to a revaluation of established categories, is itself an outcome of such a revaluation; and the view (von Uexkull) that human knowledge 'is in no way singular as compared with that of the sea-urchin, the fly or the dog', overlooks the fact that man is the only creature to first build up a body of *objective* knowledge from the data of *subjective* individual experience, and then submit this body of knowledge to systematic analysis, in which its principles of organisation are brought up into consciousness as a new and more powerful kind of knowledge.

ANNOUNCEMENTS

THE ANNUAL CONFERENCE OF THE PHILOSOPHY OF SCIENCE GROUP

THE second annual conference of the Philosophy of Science Group of the British Society for the History of Science was held from the 20th to the 22nd September 1957 at Wortley Hall, University of Nottingham, under the Chairmanship of Professor R. O. Kapp, chairman of the Group, and was attended by about fifty members and guests. Dr E. H. Hutten was conference secretary and Dr M. B. Hesse acted as conference treasurer.

Symposia were held on 'The Logic of Probability', 'Observation and Experiment', 'Homeostasis' and 'The Mind-Body Problem'. In the first symposium papers were read by Dr J. Bronowski and Dr W. Mays and Mr W. C. Kneale took the chair. Dr Bronowski discussed different senses of probability. In the main sense that he discussed probability is concerned with laws and *not* with isolated events: it is inference probability. There is no meaning to the question 'what is the probability of the sun rising tomorrow?' Perhaps it would be better to drop the term 'probability' in the context of induction and to say that a law is reasonable rather than probable. However, the establishment of a law is not like drawing a sample from the universe nor is it done by repetition only. Both procedures are not scientific induction. What we do is to estimate that the numerical parameter of a law lies within certain limits. The null-hypothesis is simply a law in which the parameter is put at zero value and so we test whether the parameter significantly deviates from this value. Fisher restricts the parameter to the interval between certain confidence limits and assumes an *a priori* probability distribution for it. Dr Bronowski explained that *in fact* we never know such a distribution. Neyman and Pearson thought that if we can establish an hypothesis with a probability of 95 per cent then we could be certain to the same extent of what the probability distribution is within a confidence interval. This is not true; for significant tests do not represent a probability distribution, i.e. to have 95 per cent confidence (or to be only 5 per cent 'out') is not the same as to score 95 per cent success in repetition. It is in the nature of things that you cannot assign a number to an inductive inference. The choice between one or other new law is based on whether its coherence with established laws is better or not. The recent experiment regarding parity and nuclear reactions is an example. We have a prejudice in favour of symmetry; therefore we are astonished when the parity law is broken. Its coherence with the laws of quantum mechanics is different from what we assumed naively. Relativity Theory is not a correction to Newtonian mechanics but a new organisation of experience in a different pattern of coherence. You cannot compare two different organisations of this kind and assign a number to each of them. Dr Bronowski suggested that a coherence estimate may possibly be made by means of a topological measure. Of two theories, that one is preferred which is most closely integrated. Thus general relativity is better than Newton's theory, for it can not only describe all the phenomena that classical theory can, but over and above this it establishes new relationships between them. Dr Mays discussed the pragmatics of probability and described the work of A. Naess on the experimental semantics of probability. Subjective probability was found to have the same

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properties as objective probability. The calculus of probability can be regarded as a model of the maturity of chances. Chances have to be learnt and the work of Piaget shows that the search for regularity is abandoned in children as they mature, probability judgments then being substituted. A lively discussion followed these two papers.

The second symposium took place on Saturday morning with Professor H. Dingle in the Chair. Dr Mary Hesse criticised the 'dictionary theory' of some philosophers of science on the ground that if the meaning of a theory depends solely on its translation into phenomenal statements whose meaning is independent of the theory, the theory can never be tested. Mr P. Alexander defended the 'dictionary theory' by maintaining that the meaning of phenomenal statements is independent of the theory, for the theory explains them, and would not do so if it were pre-supposed by them. Mr R. F. J. Withers discussed the relation between observation and experiments in certain problems in biology. In the Golgi controversy much argument is generated by workers thinking that observation alone will give them information about the structure and possible function of the Golgi apparatus; in other words they assumed that observation alone enables them to choose between different interpretations of the phenomena. Yet in the absence of a theory concerning the functioning of the Golgi apparatus no experimental work is carried out and no interpretive choice can be made. The controversy was discussed in the light of the classical epistemology of 'sensible' and 'physical' objects. By means of these terms Mr Withers maintained that sense could be made of the controversy over what is *artifactual*. During the discussion there was some clarification of levels of observation statements: theories are not explicitly reducible to observation and the *truth* of the theory which explains them must not be presupposed, although some lower level theories may be, and the *language* of the explanatory theory must be.

Professor R. O. Kapp took the chair in the Saturday evening session. Professor A. C. Mace criticised Freud's homeostatic concept of biological systems as those which tend to abolish stimuli and wished to distinguish between genuine and goal-directed systems and those which are satisfactorily explained by causal laws. Dr N. Walker exposed the inadequacies of current ideas of homeostasis by pointing out that they are too all-embracing. For example no distinction is made between static and dynamic homeostasis. A better term to use here might be 'isonomy'. The popular notion of homeostasis has become something like a final cause. This is due to the idea that the central nervous system has the sole function of abolishing stimuli or that there is only one kind of stimulus, namely the stimulus of abolishing itself. Homeostatic systems are very different from what we normally call a 'system'. We have already a complicated system when we take a pencil and try to place it on its end on the table. Adjustment as a homeostatic process may require us to envisage end states that cannot be observed. Such states may only be such that they last longer than any other, for example, the egg may be a final state rather than the hen. There is a set of covarying variables. At least some of them vary according to a certain pattern. The roulette wheel tends to maintain relative frequency but it still leaves us with a desire to find out the exact mechanism of the homeostasis. Dr Walker believed the more precise definition of homeostasis is needed. The homeostatic system may involve negative feed-back, damping, underdamping, coarseness of response, communication, amplification, reaction time, etc. This criteria was mentioned in the discussion that followed.

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On Sunday morning the symposium on the Mind-Body problem took place and its chairman was Dr E. H. Hutten. Professor Woodger suggested that an approach to the mind-body problem might be made by considering the sentences of the form x is getting y of z . This functor was a three-term relation whose first domain (D_1) consists of persons, its second domain (D_2) consists of sensible objects and its third domain (D_3) of physical objects. Professor Woodger implied that much of the classical discussion of the Mind-Body problem could take place with greater clarity by using this functor. He pointed out that physical science dealt mainly with statements in D_3 and he felt that there was an urgent need to examine the relationships between statements in D_1 and D_3 particularly in the medical sciences. Mr C. K. Grant made a detailed analysis of the various philosophical assumptions involved in Professor Woodger's approach. He discussed the relationship between public and private knowledge and whether D_1 , D_2 , and D_3 were as independent as Professor Woodger thought. The discussion showed that Professor Woodger's approach was novel and many speakers argued about the possible entities that might be put into the various domains of his functor.

The conference closed with a general discussion during which the hope was expressed that a third annual conference would be held during September 1958 and possible subjects for this were suggested.

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PHILOSOPHY OF SCIENCE GROUP CONFERENCE

The Third Annual Conference will be held at St Hilda's College, Oxford from 19th to 21st September 1958. Among the speakers will be Professor T. W. Hutchison, Dr G. Buchdahl, Mr E. Gellner, and Dr H. R. Post-Paneth. The Conference fee will be £3 12s. for full residence. Further details and registration forms can be obtained from Miss M. B. Hesse, University College, London, W.C.1.

DAEDALUS

The American Academy of Arts and Sciences have launched a new quarterly publication called *Daedalus*. The first number contains articles on science and the modern world view, of interest to our readers. These articles will be found listed among publications in the Philosophy of Science. We wish the new Journal every success in the future.

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TWO EVOLUTIONARY THEORIES (II) *

MARJORIE GRENE

8 *A Choice of Two Closed Systems?*

There are, then, two ways of looking at the evolutionary record. The disagreement between the two is not, chiefly, about verifiable or falsifiable matters of fact, but about the concepts through which these facts are to be interpreted. Each constitutes, for its proponent, a closed interpretive system *in* which he sees the facts. In such a case, the objections of one to the arguments of the other are, from the other point of view, irrelevant. Can we weigh two such systems, cognitively, at all? Or are they like an ambiguous figure which we could see one way or the other, so that we may simply take our choice? In a sense, they are: for reflecting on the evolutionary record, as biologists agree in telling us about it, we may think of it in either of these ways.¹

Thus we may seek to lose ourselves in what Simpson describes as 'great groups of animals living their history in nature'.² Or we may look rather at the ascending levels of organisation into which the continuous stream of protoplasm has shaped itself. So far, then, the two theories do seem to provide alternative frameworks for understanding the data, and it seems to be purely a matter of choice which we prefer. With respect to some details within each theory, this is in fact clearly the case. For example, it is really a matter of choice whether we say that the higher categories come before the lower, or conversely, the lower before the higher. In fact, each of our two scientists, in this connection, explicitly concedes the other's position,

* Part I of this paper appeared in the August number

¹ We may also, of course, think of it in other ways; I am not suggesting that these are the only two; they are the two I have been looking at in this study.

² *Major Features of Evolution*, p. 265

Simpson admits that *Archaeopteryx* was already a bird,¹ and Schindewolf admits that though *Archaeopteryx* was definitely a bird, if there had never been any more birds nobody would have known it.² Thus, on this particular point, each system can comfortably assimilate a theorem from the other, without endangering its total structure. So far as this particular case goes, we really can look at the matter either way.

For some details, on the other hand, one point of view seems preferable, while the other fits other cases better. Looking at the two, we may find, in this case, the choice more difficult. For example, the gradual change from one extinct species of elephant to another (marked by an increase in lamellar index, i.e. in the number of enamel plates per 10 mm. of tooth) is well handled by Simpson's purely phylogenetic taxonomy.³ It is difficult to see, on the other hand, how the origin of mammals could be handled in these terms.⁴ Perhaps what we need, then, is a more inclusive theory, which will assimilate adequately both sides of the ambiguity. What would be the requirements of such a theory? This is a difficult question, and one with grave philosophical implications; I can only suggest here a starting point for a possible answer.

9 *Philosophical reflections*

As philosophers reflecting on scientific explanation, in this case on theories of evolution, we have ourselves to stand somewhere. We have to formulate our reflections within a framework of our own. I would specify, in my own reflection on these or any other evolutionary theories, three criteria, one methodological and two ontological, in relation to which, I believe, such a theory should be assessed.

¹ That is, it had feathers; but was 'as reptilian as avian throughout' (ibid. p. 370). For a more general 'concession' on higher categories, see page 350: 'In these usual cases it is true that occupation of the zone, which in retrospect is the origin of the higher category, precedes the origin of numerous genera, species and other units that come to comprise the higher category. In *this sense*, and this only, we can agree with Wright . . . that "there seems to be a large measure of truth in the contention of Willis and Goldschmidt (also Schindewolf, G. G. S.) that evolution works down from the higher categories to the lower rather than the reverse".'

² *Grundfragen der Paläontologie*, p. 453

³ *Major Features of Evolution*, pp. 387-388

⁴ See e.g. *Grundfragen der Paläontologie*, pp. 250-251. See also the papers by Dalcq referred to in n. 1, p. 123

TWO EVOLUTIONARY THEORIES

(a) *Methodological*. Each of our two palaeontologists, we found, accused the other of similar fallacies; and in particular each accused the other of arguing in retrospect. This, I think, gives us a clue to the necessary method in evolutionary theory. For this is where they are both wrong, or both right, if you prefer. Each one does indeed argue in retrospect but this in itself is not a fallacy. For what else could they do? Thucydides could not have written the *Decline and Fall of the Roman Empire*, nor could a pre-Cambrian organism, or almost-organism, have stated a theory of evolution. This is not just trivially true. The judgment of the scientist is an essential element in every scientific explanation, and especially so in a theory of evolution, since here the scientist is judging not simply the world but the world including himself and his kind, as the world that produced himself and his kind. The French zoologist Vandel has formulated this principle for evolutionary theory as the 'Method of Recurrence'.¹ It is nonsense, he says, to decry anthropocentrism in evolution; there is nowhere to start except where we are. This is, I believe, an essential principle for evolutionary theory, as for all historical disciplines. There is no such thing as total objectivity in historical statements; we can only look back and evaluate the evidence from where we are. All historical discourse, including evolutionary theory, is in this sense reflective. In evolutionary theory, moreover, it is ourselves, including our capacity for historical reflection, that we have to explain.

(b) *Ontological*. Looking back from where we stand, therefore, at the vista of evolution, what is it that we see? Here, I think, both in the light of the biological evidence (as a layman, and as some biologists see it), and of everyday experience, we ought to admit two principles which should *both* be taken just account of in evolutionary theory. First, we should acknowledge the continuity of life, and for that matter, once embarked on an evolutionary retrospect, of life with matter. Secondly, we ought also to acknowledge the diversity of ordering principles which appear at numerous levels in this continuous process. New principles of organisation appear to have arisen out of conditions that in themselves stretch back continuously—yet they are principles of organisation which order the conditioning continuum in a

¹ A. Vandel, *L'Homme et l'Évolution*, Paris, 1949 (2nd ed. 1958). Vandel's Method of Recurrence is, in effect, an application to the problem of evolution of Polanyi's 'fiduciary programme'; see M. Polanyi, *op. cit.* pp. 264-268, and cf. also the argument on evolution in Part IV, Chapter 13 of the same work, pp. 381-405

MARJORIE GRENE

This duality, whatever the difficulties of stating it in some form to any adequate assessment of evolution.

As to this duality, I submit, our two theories are in a tension. Therefore, as philosophers reflecting on it, we can and should allow them a different weight. Neo-Darwinism, I said earlier, is towards the reduction in the sense of reducing it to its conditions. The aim was to shake life out of its rigidity and thus to set it free. It obeyed also other passions of the modern age: the reduction of principles to the least possible number, the reduction in terms of the least common denominator, of roles, conditions rather than reasons. Life is all reduced differently.² Even those thinkers, oddly, who pressed the term 'emergence', Lloyd-Morgan and others, insisted on weighting that concept towards the mechanical, so that it really is hardly emergence at all.

It is in this very reductivism that Darwinism has, in spite of its, its convincing power. For what it provided, and what it gave, was an explanation of evolution in terms of the mechanistic model of science. A mechanistic explanation must be both simple and automatic. Darwinism satisfies both these demands.

Firstly, that it unites all the relevant phenomena under this hypothesis is conceived in simple terms of existence. Secondly, it is automatic, since its one model is that of organisms mechanistically, and their evolution as a result, on the one hand, by random errors (which are the sort of things that can happen to machines despite human control), and, on the other, by the blind and automatic selection, an automatism that goes on happening by itself. It seems likely that the machine model in classical science derives much of its authority from these qualities of logical automatism. Certainly this has been so outside physics. It is in our ability to build, in other fields, theories of this character that has motivated new schools of thought in science, and philosophy. And it is this faith that

, Part IV

as of Simpson's use of 'organisation', see A. Dalcq, 'Le mot est-il près d'être résolu?', *Ann. Ste. Zool. Belg.*, 1951, 82,

TWO EVOLUTIONARY THEORIES

supports neo-Darwinism, permitting it to carry, without stumbling, the vast epicyclical superstructure which (as Professor 'Espinasse has pointed out)¹ has been erected upon it. It is this faith also which carries the Darwinian conception past difficulties like those put forward by Schindewolf, without conceiving of them as difficulties at all. In fact, it cannot so conceive them. For to take them seriously as difficulties would be to endanger precisely the logical simplicity and automatism of the neo-Darwinian framework, the very qualities which make it, to its adherents, so *scientific* an explanation. Thus, for example, Schindewolf's view of the sudden origin of basically new types or patterns of organisation implies that the recognition of order, and of novel order, is distinct from the statistical manipulation of the conditions producing order. This, however, is to introduce a duality of logical levels: continuous and small-scale conditions, *versus* discrete and comprehensive pattern; and that means to destroy the *unitary* character of the explanation. Again, to admit orthogenesis, or spontaneous direction in evolution, would be to deny the constant co-variance of gene pool and environment, and thus to suggest as a third factor an inner dynamic in organisms, as distinct both from the non-directive control of random variation and the external steering 'mechanism' of natural selection. But such a suggestion would deviate from the belief in the *automatism* of evolution. And again, it is precisely the double automatism of gene fluctuation and natural selection that *makes* the neo-Darwinian explanation a scientific explanation in the mechanistic sense. To say, therefore, that the 'facts' suggest spontaneity in the origin and development of organic pattern is, for convinced Darwinians, not to offer scientific evidence at all, but to step outside the bounds of science. From the Darwinian point of view such 'objections', which can be formulated only in non-mechanistic language, lie for that very reason beyond the scope of science altogether, and cannot therefore be taken seriously as scientific objections. Thus Sir Ronald Fisher can say in good faith that the only rational theories of evolution are those which make its driving force consist in progressive adaptation. These are the only theories, he says, 'which make at least the most familiar facts intelligible to the reason'.² They are indeed the only theories which rest on the ultimate values of total

¹ Paul G. 'Espinasse, 'On the Logical Geography of Neo-Mendelism', *Mind* (N.S.), 1956, 65, 75-77

² Sir Ronald Fisher, 'Measurement of Selective Intensity', *Proc. Roy. Soc. B*, 1936, 121, 58-62

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logical simplicity and automatism: within the mechanistic tradition, that is to say, the only rational theories.

The question remains, however: are the criteria of mechanism (logical simplicity and automatism) adequate as tests of a theory professing to interpret living things, much less the evolution of a living thing? Simpson, like many mechanistically minded biologists, boasts of following Ockam's razor,¹ which forbids us to multiply entities beyond necessity. As they apply this principle, however, biologists appear to take it to mean that entities must not be multiplied at all. There must be only one logical level, of physico-chemical events automatically directed this way or that by some equally material relationship. Yet a one-level logic of explanation appears to be unobtainable even in physics and chemistry. In a review of von Laue's *Theorie der Supraleitung*, for example, Professor H. Fröhlich writes:

'... even an attempt to derive the properties of ideal gases would find the atomic physicist (if assumed to have no knowledge of macro-physics) at a loss without the introduction of new physical concepts. He would probably start with a discussion of the motion of two and then of three weakly interacting particles, and afterwards be led to the conclusion that consideration of more particles is very complicated and unlikely to lead to any simple results. It is only after the introduction of new physical concepts which do not exist in atomic physics—such as pressure and entropy—that other simple laws of physics (the gas laws) can be found.'²

If this situation prevails in physics, so much the more so does it obtain in biology. Professor Rosenfeld has recently suggested that biologists should admit, in the use of concepts like 'order' (of which Schindewolf's 'Bauplan' or 'type' is an example), their reliance on a logic of complementarity: complementarity holding between concepts of biological organisation and explanations of the physico-chemical conditions of their existence.³ In the evolutionary context, this general relation becomes the logical duality of new comprehensive ordering principles and the antecedent particulars which were the conditions of their emergence. Such duality is, I believe, unavoidable in evolutionary theory, and should be frankly acknowledged as such.

When, on the contrary, it is not acknowledged, the result is a reductivism which, however brilliant, is basically inconsistent. Some

¹ *Meaning of Evolution*, p. 139

² H. Fröhlich, *Nature*, 1948, 161, 37

³ L. Rosenfeld, 'Causalité statistique et ordre en physique et biologie', *Anais da Academia Brasileira de Ciências*, 1954, 26, 47-50

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concept of biological organisation, which implies a non-unitary logic, is indispensable to biological theory. Where, therefore, it is ostensibly dispensed with, it nevertheless creeps in again, either in the unformulated presuppositions of the biologist, or in some ingeniously contrived disguise. This kind of procedure: the pretence of understanding a subject matter in terms of fewer and narrower concepts than are in fact indispensable to its understanding, Professor Polanyi has called the fallacy of *pseudo-substitution*.¹ It can be found, I think, at a great many places in the neo-Darwinian literature, and at a number of crucial points in the argument of *Major Features*. I want here to point out one such instance, which is especially instructive in comparison with Schindewolf, that is, Simpson's procedure with respect to the concept of types. 'Type' or 'Bauplan' is, as we have seen, a fundamental conception for Schindewolf. In terms of axiomatics, it is a primitive, undefined idea, on the initial understanding of which his theory rests. For Simpson, it is a term to be avoided. In the *Meaning of Evolution* (1949), where he uses the word freely, he explains that this is popularising shorthand, and when he says 'type' he is not in fact referring to types at all.² In *Major Features* he is more careful; his use of the term, or of its equivalent, is, it seems to me, carefully devised so as not to seem to mean type although in fact it does so. He gives us first a panoramic view of a continuum of living forms varying continuously in adaptation to a continuously changing environment. Out of this doubly flowing stream, he then distinguishes with some, but, not entire arbitrariness, various 'types of adaptation' or 'adaptive types'.³ This is, so far, not a phylogenetic concept, nor a morphological one, and if applied strictly could not produce the results Simpson wants from it. Thus, considered purely in terms of *adaptive types* whales would be fish. To prevent this, there must be some structural, some 'type' reference, implied along with the adaptive one. However, with the continuous stream of life-environment relations as our fundamental concept, we can think of adaptive types as *zones* (the two terms are from here on used as equivalent) cutting themselves out at 'ecological thresholds' in this continuum,⁴ and forget, for the moment at least, the implied structural aspect of the original conception. Since these 'zones' crystallise out of neighbouring areas in the biological continuum they will in fact be close relatives, so that the whale/fish problem will not arise imaginatively even though in strict

¹ M. Polanyi, op. cit., pp. 169-170

² *Major Features of Evolution*, p. 200

³ *Meaning of Evolution*, p. 7

⁴ *Ibid.*, pp. 201 et seq.

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logic it should have arisen. Such 'zones' are, moreover, Simpson explains, only figuratively 'zones'; they represent, he says, 'a characteristic reaction and mutual relationship between environment and organism, a way of life and not a place where life is led'.¹ But calling them zones, we can think of them almost as places, as being 'entered' and 'occupied'. Thus the need to separate out the organism as an entity, and *a fortiori* the segmentation, the 'type' aspect, is reduced and concealed.

Yet 'adaptive zone' is introduced as a *synonym* of 'adaptive type'. Although the 'type' aspect is blurred, it is nevertheless taken for granted, for the theory would not work without it. Adaptation pure and simple is an insufficient instrument for the separation of different kinds of animals or plants: as, once more, in the case of whales and fish. In practice, of course, Simpson does recognise types—general patterns of structure or function—all along. Even to count the number of enamel plates per 10 mm. of an elephant's tooth means recognising some kind of organisation of jawbones and teeth. To notice the threshold at which the artiodactyl and perissodactyl limb separated out demands knowing horses and elephants from antelopes and cows. Moreover, the concept of 'adaptive zone', by providing a more elastic substitute for 'type', allows us to forget the existence of such comprehensive knowledge.

10 Conclusion

We should conclude, I think, that where concepts of more than one logical level are necessary to the interpretation of a set of phenomena, we ought not to pretend to be operating on one level only. In the context of evolution where we in fact acknowledge novel operational principles, we should not pretend that nothing is there but the conditions without which (admittedly) they could not operate. The sum total of necessary conditions for the coming into being of an individual, a species, a phylum or of life itself are not logically or historically identical *with* the individual, or species, or phylum, or life itself. The pretence that the whole story can be told in terms of conditions alone, that flux can generate form, that materials not yet organised can logically account for their own organisation, is a pseudo-substitution, which purports to do without what it cannot in fact eliminate.

This situation is, I believe, unavoidable in a reductive theory of evolution, a theory which attempts to treat mechanistically a subject

¹ *Major Features of Evolution*, pp. 201-220

TWO EVOLUTIONARY THEORIES

matter radically incapable of such treatment. I suggest, therefore, that instead of Ockam's razor we might adopt as a test of theories of evolution the opposite principle: that entities, or more generally perhaps aspects of reality—for principles of organisation are not entities, though they do define entities—should not be *subtracted* beyond what is honest. In the light of this principle, we should ask of any theory of evolution, does it pretend to do without concepts which in fact it does not do without?

Here Schindewolf's theory, I think we must conclude, is more adequate than Simpson's. The concept of type, or of organisation in some form, is indispensable, and if banished makes its way in again, or has been there, surreptitiously, all along. Secondly, the concept of novelty or originality, which again Schindewolf admits as a fundamental concept, is indispensable. Simpson's theory, being more complex and abstract, would appear at first sight to cover a wider range of phenomena; yet by the reductive nature of its abstractions it also overlooks essential aspects of the phenomena. Schindewolf's theory is simpler in its reasoning, and less unified in its conceptual structure, but, perhaps for that very reason, it remains closer to the phenomena and does more justice to their experienced complexity.

Finally let me suggest briefly, the bearing of this particular study, limited though it necessarily is, on the epistemological implications of the problem of evolution. The problem of evolution is to explain how new forms of life have originated. But new forms of life embody new operational principles, and these must be recognised as such before we can so much as ask about the continuity of the conditions out of which they arose. True, they could not have arisen without those conditions, but neither are they, logically or ontologically, identical with them. This means, I suggest, that we should apply to evolutionary theory an historical form of Kant's transcendental method. As latecomers in evolution, we should ask: what are the necessary presuppositions of the history we believe we are? What are the minimal concepts through which we can assess that history as having happened, and without which we could not understand that it did happen? If, as philosophers reflecting on evolution, we wish to ask this question, we shall find in Schindewolf's theory at any rate nothing incompatible with asking it, whereas if we commit ourselves to neo-Darwinism, we shall be unable to understand, much less to undertake, such an enquiry.

CAUSAL EXPLANATIONS IN NATURAL HISTORY * †

T. A. GOUDGE

THERE are certain empirical sciences such as geology, palaeontology, and biology whose subject-matter is partly historical. They investigate phenomena which originated and existed for varying periods of time in the remote past, long before the appearance of men on the earth. One aim of these sciences is to describe as fully as possible each of the phenomena in question by utilising available evidence. Often, however, this evidence is fragmentary. Hence a particular phenomenon may have to be reconstructed by supplying conjectured details to fill in the gaps. But the sciences concerned with natural history do not always stop here. In addition to describing, they also formulate causal explanations of the phenomena with which they deal.

Some philosophers of science seem to me to have done less than justice to this subject. Thus Toulmin takes the position that there are no causal explanations at all in natural history, and little that deserves to be called 'explanation' in any sense. The sciences concerned, he thinks, are restricted to making descriptive generalisations based on the observation of elementary regularities.¹ Braithwaite, on the other hand, agrees that biologists and others sometimes ask historical questions about the origin of particular events, and seek to give causal answers to those questions. But he holds that 'to ask for the cause of an event is always to ask for a general law which applies to the particular event'.² In the present paper I want to suggest some reasons for disagreeing with both of these positions by showing (a) that causal explanations do occur in natural history, and (b) that some, at least, have logical features which differentiate them quite sharply from causal explanations which apply a general law or set of laws to particular events.

* Read at a meeting of the Eastern Division of the American Philosophical Association, 28 December 1957.

† Received 21. i. 58

¹ S. E. Toulmin, *The Philosophy of Science*, London, 1953, pp. 44 sqq.

² R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, p. 2

CAUSAL EXPLANATIONS IN NATURAL HISTORY

I

Sciences concerned with natural history seek causal explanations for two main types of phenomena. The first type consists of the origin or genesis of single objects, particular kinds of objects, and particular states of affairs. Thus explanations are sought for the origin of the moon, the origin of reptiles, the first occupation of dry land by the vertebrates, and so on. The second type of phenomena consists of the disappearance of single objects, or particular kinds of objects, and the termination of particular states of affairs. Thus students of natural history try to explain why the dinosaurs or the Irish elk became extinct. I will restrict my remarks to these two types of phenomena, and will focus attention only on specimens of explanation found in the biological sciences.

When a biologist investigates a phenomenon of natural history, what he frequently wants to explain is an individual occurrence, something which has 'come to be' or 'ceased to be' *just once*. The phenomenon is therefore non-recurrent, and hence not subject to controlled experimentation. The precise circumstances under which it arose or disappeared, including the temporal interval involved, cannot be duplicated. Its spatio-temporal boundaries are impossible to specify exactly. Nevertheless, the aim of a biologist may very well be to account for the genesis or the termination of *each* such individual phenomenon. To accomplish this aim he may try to reconstruct the causal conditions which were operative, and to arrange them in an explanatory pattern.

Now it is generally agreed that from a strictly logical standpoint the causal explanation of any phenomenon, P, ought to conform to the requirements of one or the other of two schemata. Either (a) the explanation ought to indicate those conditions temporally prior to (or simultaneous with) P which are independently necessary and jointly sufficient for the occurrence of P; or (b) the explanation ought to indicate those conditions temporally prior to (or simultaneous with) P which are jointly sufficient but not independently necessary for the occurrence of P. The second case obviously allows for the possibility that there may be a disjunctive set of sufficient conditions of P ('plurality of causes'). Yet each member of this set has to include all the necessary conditions of P together with enough contingent contributory conditions to permit a sufficient condition of P to be constituted. In actual practice, both of these schemata represent ideals, rarely, if ever,

attained even by the so-called 'advanced sciences'. The causal explanations with which I am concerned are at best approximations to schema (b). That is to say, they are explanations which seek to formulate a possible sufficient condition of P by specifying as many of the necessary and contingent contributory conditions of P as can be discerned or imagined. Sometimes the sufficient condition is first entertained in a vague form, and then its vagueness is gradually reduced by 'filling in' a number of the necessary and contributory conditions which it embraces. This also increases its testability, as I will try to show. But the strict requirements of schema (b), let alone (a), are never met. Hence a large degree of flexibility attaches to the notion of causal explanation in natural history.¹ The particular pattern which I propose to consider here is, therefore, not put forward as the *only* one which natural history exhibits. I have no wish to construct a Procrustean bed on which all explanations have to be fitted, especially in a paper which is to a large degree exploratory.

2

Let us now look at an actual specimen of causal explanation in natural history. It is advanced to account for a particular occurrence, namely, the occupation of dry land by the first vertebrates, the primitive amphibians which inhabited fresh-water pools and streams in Carboniferous and Devonian times, and which possessed rudimentary limbs. I will display the specimen at some length before commenting on it.

'Why should the amphibians have developed these limbs and become potential land-dwellers? Not to breathe air, for that could be done by merely coming to the surface of the pool. Not because they were driven out in search of food, for they were fish-eating types for which there was little food to be had on land. Not to escape enemies, for they were among the largest animals of the streams and pools of that day.

The development of limbs and the consequent ability to live on land seem, paradoxically, to have been adaptations for remaining in the water, and true land life seems to have been, so to speak, only the result of a happy accident. . . .

The Devonian, the period in which the amphibians originated, was a time of seasonal droughts. At times the streams would cease to

¹ Cf. F. A. Hayek, 'Degrees of Explanation,' this *Journal*, 1955, 6, 209-225. I am indebted to certain of the ideas expressed in this paper.

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flow. . . . If the water dried up altogether and did not soon return, . . . the amphibian, with his newly-developed land limbs, could crawl out of the shrunken pool, walk up or down the stream bed or overland and reach another pool where he might take up his aquatic existence again. Land limbs were developed to reach the water, not to leave it.

Once this development of limbs had taken place, however, it is not hard to imagine how true land life eventually resulted. Instead of immediately taking to the water again, the amphibian might learn to linger about the drying pools and devour stranded fish. Insects were already present and would afford the beginnings of a diet for a land form. Later, plants were taken up as a source of food supply. . . . Finally, through these various developments, a land fauna would have been established.¹

This piece of reasoning has a number of distinctive features. Although it is part of a scientific discussion, it is not concerned to discover or to confirm a general law. It does not establish any new empirical fact which is simply to be added to the store of human knowledge. It does not make an explicit, positive prediction about what will be found by future investigations, though there is a sense in which it makes certain negative predictions, as I will suggest below. What the reasoning does is to propose a theoretical pattern composed of accepted statements of facts ('The Devonian . . . was a time of seasonal droughts'), statements inferred from these statements of facts ('At times the streams would cease to flow . . .'), statements which are plausible conjectures about various occurrences ('The amphibian . . . could crawl out of the shrunken pool, walk up or down the stream bed . . . learn to linger about the drying pools and devour stranded fish . . .'), etc. This pattern presents an intelligible sequence of events such that the phenomenon to be accounted for 'falls into place' as the terminal phase of the sequence. The phenomenon ceases to be isolated, and is connected in an orderly way with states of affairs which led up to it.

Looking more closely at the reasoning, we can say that what it does is to formulate by means of the theoretical pattern a possible sufficient condition of the phenomenon to be explained.² This condition is both

¹ A. S. Romer, *Man and the Vertebrates*, 3rd edn., Chicago, 1941, pp. 47-48

² To say this is, of course, to give an analysis or reconstruction of the reasoning in the light of a logical schema. It might be argued that what the example cited really offers is a 'teleological' not a 'causal' explanation. Yet if one takes the explicandum to be contained in the question, 'Why did the primitive amphibians become land dwellers?' the explanation makes no reference to purposes, ends, or

complex and formulated in broad terms against a background of assumptions, theories, and information which serves to limit it. The explanatory pattern has specifiable 'boundaries' which allow it to be presented as a relatively isolated unit. Thus, although the explanation of the origin of land fauna presupposes the origin of vertebrates, we do not have to explain the latter phenomenon in order to explain the former. The pattern specifying the possible sufficient condition involves a number of components which the reasoning undertakes to enumerate. These components are either statements of necessary conditions (e.g. that the amphibians must have emerged from the water to become land dwellers, that they must have found suitable food after they emerged, etc.), or statements of contingent contributory conditions (e.g. that the amphibians walked up or down stream or overland, etc.). It is fairly evident that no exhaustive list of the component conditions is given. It is also evident that where conjectured events are introduced into the pattern, their plausibility depends on the relevance they have to the over-all sufficient condition. Without the latter there would be no basis for deciding what events can be plausibly included in the pattern and what events must be excluded. This is the main, though not the only, controlling factor in the situation.

It is tempting to think of such a causal explanation as conforming to the model of a deductive system. One is inclined to feel that the statements making up the explanation must be capable, at least in principle, of being organised deductively or perhaps even axiomatically. But this seems to me to be the wrong model to have in mind. As the above example shows, an explanatory pattern in natural history is closer to the model of a *coherent narrative* than to that of a deductive system. The narrative 'pictures' a certain temporal sequence of events. But the sequence is not such as to allow each event to be deductively inferred from its predecessors. For the events specified do not constitute just the necessary conditions of the phenomenon, and certainly do not constitute, in most cases, all those conditions. As a rule, only the necessary conditions which are 'crucial' or 'most

goals. No biologist is likely to suggest that the activities of the amphibians were 'directed' towards the establishment of land fauna. This result was only 'a happy accident'. Even if we include as part of the explicandum the question, 'Why did the amphibians develop land limbs?' we cannot give the 'teleological' answer, 'In order to be able to live on the land,' but have to say, according to the example, 'In order to remain in the water'. A causal reconstruction of the reasoning seems, therefore, to be what is required.

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important' are stated. Along with them are included a variety of contingent contributory conditions. Hence, given the determinate temporal order of the events pictured by the explanatory pattern, the logical relations among many of the component statements are merely conjunctive, not implicative. I do not deny, of course, that implicative relations do obtain between *some* statements of the pattern. But this is clearly not enough to yield a deductive system.¹

The suggestion may be advanced that what we are concerned with here is really a causal chain of events. The temporally successive stages represented by the pattern make up a linear series, each member of which is the effect of its immediate predecessor and the cause of its immediate successor. The first 'link' of the chain is an event whose predecessor we disregard; and the final 'link', being the event we want to explain, is one whose successor we disregard. This analogy of a causal chain is, however, quite unsatisfactory, as N. R. Hanson has recently shown.² In the present case the analogy tends to obscure the fact that each component of the pattern is specified so as to be compatible with the existing body of scientific knowledge, and would be unintelligible apart from that knowledge. Furthermore, the analogy falsely suggests that the specification of the components is exhaustive.

¹ Hence the model of a hierarchical deductive system, so often presented as the ideal to which the theoretical part of *every* science should approximate, is not relevant to the sciences concerned with natural history, however much it is relevant to non-historical sciences.

It may be objected that the proposed reconstruction of the reasoning does not escape being deductive. For if, as suggested, the individual phenomenon, P, is explained by specifying a complex sufficient condition, S, then we are entitled to affirm the generalisation, 'Whenever S, then P.' But if so, the reasoning is clearly an ordinary mixed hypothetical syllogism: 'Whenever S, then P. But S. Therefore, P'.

To this objection several replies can be given. (i) In actual practice, S is specified as a *possible* sufficient condition of P, and hence we are *not* entitled to affirm the generalisation, 'Whenever S, then P'. (ii) Even if, after prolonged investigation, we become convinced that S was in fact the sufficient condition of P on the particular occasion in question, we should not be entitled to affirm 'Whenever S, then P' as an *empirical* generalisation. For we have just the one, non-recurrent case, P, to go on; and no biologist would wish to generalise from a single instance. It is true, that the assertion of S as a sufficient condition of P entitles us to affirm *the logical principle*, 'If S, then P'. Furthermore, this is a principle accepted by the biologist when he argues, 'P because S'. But it is not a principle from which the latter assertion follows deductively. Cf. G. Ryle, "If", "So", and "Because", in *Philosophical Analysis: A Collection of Essays*, Edited by Max Black. Ithaca, 1950, pp. 323-340

² N. R. Hanson, 'Causal Chains', *Mind*, 1955, 64, 289-311

(One cannot have a 'chain' if links are missing.) In short, the causal chain image tempts us to overlook the importance of contextual considerations here, as elsewhere. Each explanatory pattern of the sort we are considering is less like a segment of an isolated causal line stretching back indefinitely into the past, and more like a portion of a complex historical network with an enormous number of cross-connections.

The choice of the over-all pattern as a possible sufficient condition of the phenomenon is made in the light of existing knowledge or reasonable inference therefrom. In the example quoted, the initial move is to eliminate certain alternative possibilities because they are incompatible with accepted facts. (The amphibians did *not* become land-dwellers because they were driven out of the water by the need to breathe air, or to obtain food, or to escape from predators.) Another alternative (that the ability to live on land was an adaptation for continuing to live in water) is compatible with everything known. But it is formulated in such a way as to be both vague and paradoxical. To remove its paradoxical character and reduce its vagueness, a number of the component events are narrated in temporal sequence. Obviously, the selection of these events depends not only on their relevance to the over-all pattern and their compatibility with the general body of scientific knowledge, but also on the demands of internal coherence imposed by the sequential character of the total narrative. The narrative has to tell a 'likely' story. It is worth noting how much use is made throughout not of the simple past tense, but of the possibility-expressing auxiliaries, 'could', 'would', and 'might'. They show the conjectural nature of many of the constituents of the pattern.

Another side of this matter will be highlighted if we take note of a case where natural history has *failed* to find a causal explanation of a phenomenon. It is well known that no-one has been able to offer an acceptable explanation of the extinction of the dinosaurs. The difficulty here seems to be that there are *too many* possible factors available, rather than too few. Palaeontologists are unable to make a selection from the numerous alternative possibilities, and thus construct a plausible explanatory pattern. As an eminent palaeontologist puts it:

'No one knows exactly why dinosaurs became extinct. This is not because there is anything mysterious or metaphysical about extinction or because possible causes are unknown. It is just because there are many reasonable possibilities, and the record does not enable us to say in the particular case which of them were actually involved.'

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All we can say at present is that something changed and the dinosaurs did not.¹

Note that it would be considered unsatisfactory to suggest as an explanation some very general sufficient condition of the phenomenon, e.g. that dinosaurs became extinct because they failed to adapt to environmental changes. This suggestion fails to 'fit' the particular phenomenon tightly enough. To make the proposed explanation satisfactory in a minimal sense, some of its components (necessary or contributory) would have to be reconstructed in temporal sequence. Where, as in the present case, it is impossible to choose a *specific* sequence of components from the wide range of possibilities available, an acceptable explanation cannot be put forward.

3

At this point it may be objected that if the above account is correct, then the causal explanations in natural history which have been discussed all suffer from a fatal defect. They are insusceptible to any sort of empirical testing. We can, indeed, ensure that a given theoretical pattern is internally coherent and compatible with existing knowledge. But since it involves no general laws and makes no positive predictions, we can never subject it to any verificational procedure. It remains a sheer conjecture about a past event.

To this objection the reply may be made that in as much as an explanatory pattern specifies a determinate sequence of events (or conditions), it restricts the possibilities with regard to what future empirical investigations will disclose. The pattern does, therefore, involve a number of negative predictions. It implies *that certain phenomena will not turn up in any future inquiries* (e.g. in palaeontology, comparative anatomy, experimental biology, etc.). If one of these phenomena does turn up, it will invalidate or falsify the pattern. Hence, such causal explanations in natural history *are* testable, for nothing prevents their inadequacy from being detected, in the long run, if they are inadequate. Since Popper has given powerful reasons for believing that a distinguishing mark of a scientific theory is its falsifiability, i.e. the possibility of overthrowing it on the basis of empirical evidence, these causal explanations can be regarded as intrinsically no less scientific than (though in other respects quite different from) explanatory theories in the non-historical sciences.

¹ G. G. Simpson, *Life of the Past: An Introduction to Palaeontology*, New York, 1953, p. 139

This consideration provides a rule of selection in situations where more than one explanatory pattern relevant to a phenomenon, P, can be formulated. For suppose that E_1 and E_2 are a pair of such patterns; and that in the case of E_2 we are able to specify a smaller number of the component conditions (necessary and contributory) than in the case of E_1 . It follows that E_1 will then be more readily shown to be inadequate than E_2 . In other words, E_1 is a more testable pattern than E_2 , because the temporal sequence of conditions presented in the case of the former involves a larger number of negative predictions than does the temporal sequence presented by the latter. This fact should clearly lead us to choose E_1 rather than E_2 as a causal explanation of P. But I think Popper is right in affirming that no explanatory theory can ever be conclusively confirmed. It may be progressively confirmed, of course, as the result of a long series of unsuccessful attempts to overthrow it. At bottom each explanatory pattern in natural history is an 'informed conjecture' about the origin or the termination of some past phenomenon—not a categorical affirmation of the truth, but an attempt to find out the truth about it.¹

I submit, then, that natural history does employ causal explanations, and that the most typical, being the sort I have attempted to describe, do not bring an individual phenomenon under a general law. Furthermore, these typical explanations seem to me wholly appropriate to the subject-matter with which natural history is concerned. The method used in constructing them is entirely sound. There is no superior method (e.g. that of physics) which ought to be substituted for it. Hence, no warrant exists for saying that all explanations found in the historical sciences are 'weak', or for suggesting that these sciences are still at a rudimentary stage of development. Physics is not necessarily the adult form of every science, and natural history is not adolescent physics.²

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¹ The *locus classicus* of Popper's discussion is, of course, his *Logik der Forschung*, Vienna, 1935, now happily promised in an English translation, *The Logic of Scientific Discovery*, London. Cf. also his two stimulating essays, 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy*, 3rd Ser., edited by H. D. Lewis, London, 1956; and 'Philosophy of Science: A Personal Report', in *British Philosophy in the Mid-Century*, edited by C. A. Mace, London, 1957.

² For other aspects of the subject of explanation in the historical sciences, see especially: W. B. Gallie, 'Explanations in History and the Genetic Sciences', *Mind*, 1955, 64, pp. 160-180; 'In Reply to Mr Montefiore', *Mind*, 1958, 67, pp. 92-96; and R. P. Gould, 'The Place of Historical Statements in Biology', this *Journal*, 1957, 8, 192-210.

R. B. BRAITHWAITE ON PROBABILITY [AND INDUCTION]*¹

HENRY E. KYBURG, JR.

R. B. BRAITHWAITE devotes nearly half of his recent and fascinating book, *Scientific Explanation*,² to a consideration of the related problems of probability and induction. I do not find what he has to say about induction completely satisfactory, and I should like to give my reasons for this feeling and for tracing it back to his treatment of probability and statistical inference.

I

Braithwaite begins his consideration of the justification of induction with an excellent statement of the problem: 'We do not come to the discussion of how we obtain reasonable belief in a scientific hypothesis already knowing what we mean by such a "reasonable" belief and . . . such a "valid" inference; in stating the conditions which justify inferences we shall, *ipso facto* be giving criteria which determine the meaning of the phrases "valid inference" and "reasonable belief" in the case of an inductively established hypothesis.'³ This is precisely the problem: to elucidate the meanings of these honorific epithets.⁴

Braithwaite approaches his solution by proposing that we stand back and look at what all inductive inferences have in common. 'What then becomes clear is that all the inductions whose validity we wish to establish are inferences in which the inductive conclusion has been derived from the empirical premises in accordance with one or other of a limited number of inductive principles of inference.'⁵ He

* Received 27. ii. 58

¹ This article is adapted from Chapter Five of my dissertation, *Probability and Induction in the Cambridge School* (University Microfilms, Ann Arbor, 1955), which was prepared at Columbia University under the direction of Professor Ernest Nagel, to whom I am indebted for much excellent advice.

² R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953

³ Ibid. p. 257

⁴ Cf. my article, 'The Justification of Induction', *Journal of Philosophy*, 59, 1956

⁵ Braithwaite, *op. cit.* p. 260

does not want to go so far as to say that this is all there is to the solution of the problem, for it is obviously possible to call the principles of inductive inference themselves into question. He therefore formulates what he calls the *predictionist* justification of the principles of inductive inference, for which he acknowledges indebtedness to Peirce. The justification for an inductive policy (principle of inference) is that by following it we are led to hypotheses which are generally confirmed, and not refuted, by subsequent experience.

This is formulated in more precise language, and given expression in the following definition of an *effective inductive policy*:

I is an effective inductive policy if and only if,

'Of every time *t* later than a fixed time *t*₀, and of every interval of time of a fixed length of years *d* lying within the interval (*t*₀, *t*), it is true that many of the hypotheses established by the use of policy *I* during the interval of *d* years (unless there are no such hypotheses) have the joint property (1) of not having been empirically refuted at any time between the time of establishment and *t*, and (2) of having been empirically confirmed at least once between the time of establishment and *t*.'¹

An inductive policy which is *effective-in-the-past* is one which satisfies the above criterion, except that 'of every time *t* not later than the present time' is substituted for 'of every time *t*'.

Having presented these definitions, he goes on to say, 'It is a historic fact that the inductive policies of good scientific repute are effective-in-the-past policies; it is a general hypothesis that they are effective as well as being effective in the past'.² It clearly follows that '*the effectiveness of an inductive policy is an empirical proposition which does not logically follow from the policy's effectiveness-in-the-past*'.³ But the effectiveness of the inductive policy concerned is the ground on which the epithets 'valid' and 'reasonable' are applied to an inductive argument and its conclusion, respectively. 'The thesis maintained in this chapter is that the effectiveness of the inductive policy concerned is a sufficient condition for the adjective "valid" to be applied to an inductive inference from known evidence and for the adjective "reasonable" to be applied to belief in the conclusion of the inference.'⁴ Braithwaite apologises for the weakness [*sic*] of this criterion of validity in the words: 'But if all that can be said in favor of using an inductive policy is that it frequently predicts successfully,

¹ Braithwaite, *op. cit.* p. 267

² *Ibid.* p. 268

³ *Ibid.* p. 269. My italics.

⁴ *Ibid.* p. 270-1

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is this a justification for basing actions upon beliefs obtained by such a policy?'¹ After a struggle with his conscience, he decides that it is; but the implication remains that we can and ought to have a guarantee that a given inductive policy will 'frequently predict successfully'.

There is little reason to dispute the sufficiency of the criterion in question—partly because of the indefiniteness of the interval of time d , the location of the given time t_0 , and the number represented by 'many'. We *could* dispute it: A successful swami may very often predict successfully by means of his mysterious method of crystal-gazing, deep breathing, and (what we would call) free association. But the fact that many of his more indefinite predictions are vindicated does not give us any great warrant for expecting his more specific predictions to be vindicated. Braithwaite does not mention the possibility that while many hypotheses established by a given method are confirmed and not refuted by future experience, many, many more, established by the same method, are refuted and not confirmed by future experience. Would this not constitute grounds for preferring some other method?

Braithwaite implies, though he does not explicitly claim, that his criterion is necessary as well as sufficient. While the insufficiency of the criterion can be taken care of by adding a few clauses to it, the question of whether or not it can be plausibly maintained that the criterion is a necessary condition for the validity of an empirical inference, raises even more interesting problems. If we are to justify induction at all, and if the criterion cannot be satisfied, then it is clearly unplausibly stringent. Braithwaite claims that the criterion can be satisfied; he says that we can know that a given inductive policy is effective because it can, in some cases, be established as effective by another inductive policy. 'That policy Π is effective is an inductive hypothesis to be established by induction according to a principle of simple enumeration on the basis of its effectiveness-in-the-past.'²

2

We can justify belief in the hypothesis that Π is an effective policy, given (a) that Π is an effective-in-the-past policy, and (b) that the policy of induction by simple enumeration is an effective policy. So far, if we charitably ignore the details of this justification, so good. But immediately the question arises as to whether it is possible to justify

¹ Braithwaite, *op. cit.*, p. 272

² *Ibid.*, p. 273,

the inductive policy of induction by simple enumeration in this same way. Braithwaite answers this question in the affirmative; I shall go into his argument in some detail, since if it is valid, it is extremely important: any number of inductive policies may turn out to be self-justifying.

The argument is as follows: 'Let Π be the inductive policy of adding belief in a hypothesis h to belief in a set of propositions which collectively constitute the π -evidence for h , . . . π being the principle of induction by simple enumeration. Let e be the proposition that the policy Π is effective.'¹ 'The truth of e justifies the use of policy Π ; i.e. for every h the truth of e justifies an addition of belief in h to reasonable belief in π -evidence for h .'² Substituting e for h , we get: the truth of e justifies an addition of belief in e to reasonable belief in π -evidence for e . It is then claimed that this argument escapes from the *petitio principii* sort of circularity by virtue of the fact that e must merely be true—it need not, in addition, be reasonably believed, or, for that matter, believed at all. We may still ask, however, whether or not the inductive inference in question is satisfactory.

This leads Braithwaite to the general question of what it means to call an inference valid.³ He considers the case of a person B inferring q from p by means of a principle of inference whose effectiveness is asserted by r . He lists ten possible circumstances, but only the following three are relevant:

VI. B reasonably believes p and believes r .

VIII. B reasonably believes p and r is true.

IX. B reasonably believes p and believes r and r is true.

An inference which proceeds in accordance with VI is called subjectively valid; one which proceeds in accordance with VIII is called objectively valid; and one which proceeds in accordance with IX is both subjectively and objectively valid.

Braithwaite claims that in any of these three senses, the inference to e by way of Π and π -evidence for e is valid. 'To justify the use of the policy of induction by simple enumeration by its effectiveness, as we have done, is to use criterion VIII for the objective validity of the inference to the hypothesis of its effectiveness.'⁴ Since Braithwaite

¹ Braithwaite, op. cit. p. 276

² Ibid. p. 277

³ 'Cogent', I think, would be a better term. By 'valid' one generally implies only that the inference does in fact conform to the principle of inference to which it is alleged to conform; cf. my article in the *Review of Metaphysics* (September, 1958).

⁴ Braithwaite, op. cit. p. 281

has expressly denied that the effectiveness of Π is known *a priori*, this argument boils down to: *If Π is effective, then we can reasonably infer the effectiveness of Π from the π -evidence for the effectiveness of Π .* In other words, since we have abundant evidence (presumably) for the effectiveness of Π , we may say that *if Π is effective, then we may reasonably believe that Π is effective.* Although I am willing to agree that this argument avoids circularity, it is nevertheless clear that it does not answer to Braithwaite's need for a *justification* of the policy of induction by simple enumeration. We must be able to assert the antecedent, ' Π is effective' if we are to be free to assert the consequent, 'We may reasonably believe that Π is effective', by *modus ponens*. Although it may be that in fact the antecedent is true, we cannot assert the consequent unconditionally unless we can *assert* the antecedent—which we obviously cannot. Only God, who presumably knows whether or not Π is an effective policy, can know whether our belief in the effectiveness of Π is reasonable or not. All we can say unconditionally is that our belief *may* be reasonable.

For those who find this sense of 'validity' unsatisfactory, Braithwaite proposes the subjective sense, as exemplified in inferences of type VI. In this form of inference, we proceed from mere belief in the effectiveness of Π to *reasonable* belief in its effectiveness. This form of inference also avoids the *petitio principii* sort of circularity. But while the former argument can be employed only by God—and thus, in the mouths of mortals, does not prove enough—this argument seems to prove too much. It certainly does not entail that the use of policy Π will in point of fact enable us to predict successfully in the future; it states that it is reasonable (in a certain sense of 'reasonable') to believe this; this sounds plausible enough, but it would seem to be just as plausible to start out by asserting that it is reasonable to believe in the effectiveness of Π , on the grounds that this is part of what we *mean* by reasonableness. For that matter, we may assert that it is reasonable, by definition, to believe in conclusions obtained by the use of inductive policies which are effective-in-the-past, dropping all reference to future effectiveness. As it stands, the argument proves too much because it can be applied to policies which do not, to most of us, seem reasonable at all. This may be illustrated by an example.

Let r assert that the Oblonsky Method of tea-leaf reading is a highly successful method of predicting the future; let q assert that in the future the application of this method will lead to many true conclusions; and let p be the empirical proposition that the tea-leaves in my morning

cup of tea lay in such a way that the Oblonsky Method predicts q on the basis of p . If I believe in the Oblonsky Method, then by analogy with Braithwaite's type VI inference, I may *reasonably* believe q . The inference from the empirical premise p to the conclusion that in the future the Oblonsky Method will lead to many true conclusions is subjectively valid. Indeed, if the Oblonsky Method really is a highly successful way of predicting the future, the inference is objectively valid as well. The objection that the method has (in crass scientific terms) been 'refuted' in the past, is simply irrelevant: I am interested in the future successes of the method, not in its past failures. And I am just as free to employ the Oblonsky Method to refute the hypothetical effectiveness of induction by simple enumeration (at least as applied to the Oblonsky Method) as I am to employ the effectiveness of induction by simple enumeration to refute the hypothesis that the Oblonsky Method will lead to increasingly frequent success in the future. Both methods are in the same logical boat.¹

Even if we could accept Braithwaite's argument for believing in the effectiveness of the policy of induction by simple enumeration, there would remain difficulties connected with this justification of induction. The criteria for effectiveness-in-the-past are vague, involving reference to 'many' hypotheses, a span of years d , and so on. Braithwaite does not describe any inductive policies in detail, so that we are not quite sure what constitutes a 'policy'. If we interpret 'policy' in very broad terms, then we must be prepared to say that some policies are more successful in some areas of application than in others; if we interpret 'policy' in a narrow sense, then we may find that we have very little enumerative evidence in favour of the effectiveness of most of the prevalent inductive policies; if we interpret 'policy' in still narrower terms, we will find that every inductive inference that was ever made employed its own unique inductive policy. Thus the policy of inferring from a sample of goldfish collected from pond P on 30 January 1958, that the mean weight of the goldfish in pond P is w grams, has never been used before and will never be applicable again.

Furthermore, even if there is no circularity in an inductive argument purporting to support induction, there does seem to be an aura of redundancy about it. If a given inductive policy is effective, it is so in virtue of some empirical truth. If we isolate this true (or reasonably credible) empirical element, then, employing a statement of this

¹ Though even Oblonsky would no doubt hesitate to argue that his method had 'refuted' the general method of induction by simple enumeration.

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element among the premises, together with the empirical data which provide the grounds for the inference, we will have an inference which can be shown to be demonstrably credible. Thus if we can show that the statement, 'Samples of size N , of objects of type T , selected by method M , are representative of the mean weight of all such objects of type T ' is rationally credible, then surely there is no difficulty about showing that the consequence inferred from this statement in conjunction with appropriate data statements, 'The mean weight of objects of type T is W ', is rationally credible. If we can show that the empirical hypothesis is well confirmed, then we do not have to ascend to inductive *policies* at all. On the other hand, if we cannot show that the hypothesis is well confirmed, then I do not see how we can get away with incorporating it in an inductive policy; to incorporate it in an inductive policy is tantamount to assuming that it is, if not true, at least rationally credible.

Finally, there is the question of practicality. While there are many people who would like to see a general justification of induction, and while the search for such a justification is certainly interesting and worthy, there is a more immediate reason for concern over the problems of induction and of probability. Competent investigators in the sciences usually agree concerning whether or not the acceptance of an empirical generalisation is warranted by the evidence, but they do not *always* agree.¹ The canons of inductive inference in accordance with which they could always come to agreement have not been made explicit. If they were, such disagreements as there are could be avoided, barring only those which arise with respect to theories in which an intuitive sense of the meaning of the theory in relation to the entire field seems to play (if only initially) an important rôle in determining its credibility.

These last three paragraphs cannot be taken as criticism fatal to Braithwaite's point of view; all three objections could be met by a more detailed and explicit characterisation of definite inductive policies—preferably all of that 'limited number' to which Braithwaite refers. Since it was not his object in *Scientific Explanation* to carry through such a detailed analysis, he can hardly be held culpable for not having done it. But we can reasonably expect his treatment of induction by simple enumeration to be rather detailed, since it is on this policy that all

¹ The most dramatic case in point is that of the hypothetical existence of 'psychic' phenomena. A more pedestrian example is the hypothetical relationship between smoking and cancer.

of the other inductive policies must ultimately depend. Since induction by enumeration is a special case of statistical inference, I shall now turn to Braithwaite's theory of probability and his analysis of statistical inference.

3

Braithwaite gives an interpretation of probability as it is used in statistical hypotheses which is new and very attractive in its simplicity. He begins by stating that one of the natural uses of probability is for expressing a proportion: 'Instead of saying that the proportion of male births among births is 51 per cent., we may say that the probability, or the chance, of a birth being a male birth is $51/100$.'¹ This is the only sense of probability which Braithwaite considers useful in relation to the problems he is considering. 'What a philosopher of science is concerned with is, first, probability as it occurs within the hypotheses of a science, i.e. with the concept of probability as the characteristic of statistical hypotheses; and, secondly, probable in the sense of reasonable as applied to scientific hypotheses themselves or to belief in them. For the rest of this book the word will be used only for the empirical, numerically measurable concept characteristic of statistical generalisations.'²

He proposes to interpret probability as it is used in statistical hypotheses by means of a model built along the following lines:

To the probability statement that the probability of a child born being a boy is $51/100$ and to the observed class-ratio statement that among 1000 children born 503 are boys, there is to correspond, respectively, a set-up of the model with a set of 1000 bags each containing 100 balls 51 of which are black, and a draw, one from each of the 1000 bags, of 1000 balls 503 of which are black. To the proposition about the observed class-ratio there corresponds not a draw of balls forming a *subclass* of a class whose proportion of black balls is $51/100$, as in the hypothetical infinite population model, but a draw of balls forming a *selection* from a class of classes, the proportion of black balls in each of which is $51/100$. The new model allows for the possibility of a draw of increased size by increasing the number of bags while keeping the number of balls in each bag constant.³

If we denote the class of balls by β , and the class of black balls by α , we can define the α -parameter of the (α, β) -hyperclass consisting of all the individual bags, as being the class-ratio of black balls to all the balls

¹ Braithwaite, op. cit., p. 118

² Ibid., p. 121-2

³ Ibid., p. 130

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in each bag. A statistical hypothesis stating the probability that an α will be a β , can be interpreted in a model in which the (α, β) -hyperclass consists of an indefinite number of bags, each of which contains a ratio of α objects to β objects equal to the α -parameter (the probability value) assigned by the statistical hypothesis.

Braithwaite treats probability as a theoretical concept which is employed only in statistical hypotheses; to understand probability, then, we must understand how statistical hypotheses are used, and what they mean. Braithwaite accepts a modified verifiability theory of meaning, in terms of which the meaning of a statistical hypothesis is given by means of a ' k -rule-of-rejection', which directs us to reject a probability statement when confronted by evidence of a certain specifiable statistical character.

Consider a class β , a certain proportion of whose members are also members of α . Let there be a statistical hypothesis asserting that this proportion is p , i.e. that the α -parameter of any (α, β) -probability hyperclass has the value p . We draw an n -fold sample of β 's, which corresponds, in Braithwaite's model, with a selection of one ball from each of n bags, each having the α -parameter p . Given an arbitrary small number k , we are directed by the k -rule-of-rejection to reject the hypothesis if the α -ratio in the sample differs from p by more than $\sqrt{pq/nk}$, where $q = 1 - p$.

There is an argument for using this rule of rejection. It is a theorem in the arithmetic of class ratios that the proportion of selections from a probability hyperclass whose α -parameter is p , which have α -ratios differing by more than $\sqrt{pq/nk}$ from p , is less than k . Therefore, if our statistical hypothesis is true, the probability that we shall reject it is less than k . This last probability statement is also a statistical hypothesis; it concerns the proportion of n -fold samples which have α -ratios lying within fixed limits, and it may be tested by means of a higher-level k -rule-of-rejection. We make n_1 sets of n observations each; if the proportion of n -fold samples which have an α -ratio lying outside of the fixed limits $p \pm \sqrt{pq/nk}$ is greater than $k + \sqrt{1/n_1}$, then we reject the second-level hypothesis, and therefore also the first-level hypothesis. If the first hypothesis was rejected in accordance with the k -rule, but the second one is not, then we may re-instate the first hypothesis on the grounds that its rejection was atypical. This same procedure may be applied to third-level hypotheses, fourth-level hypotheses, etc., each of which has its derived k -rule-of-rejection.

It may immediately be seen from this programme that a statistical hypothesis can never be definitely rejected or confirmed. 'Although every rejection may be cancelled, yet it also may not be; and the possibility of an unending series of empirical tests, none of which reject the statement serves to give, I maintain, an empirical meaning to the statement'.¹ It should be noted that on Braithwaite's theory the *k*-rule-of-rejection does not give us any *reason* for believing or disbelieving, accepting or rejecting, a statistical hypothesis. 'The reason for disbelieving a probability statement is the empirical² knowledge on the basis of which it has to be rejected in accordance with the *k*-rule-of-rejection which determines the meaning of the statement; *that it has to be rejected on the basis of such knowledge is not the reason for disbelieving it, but is a statement of how probability statements are used*'.³ When *k* appears in the *k*-rule, it simply helps to characterise the kinds of observations which will result in the rejection of the original hypothesis. When *k* appears in the explanation of *why* we use a certain *k*-rule, then it is merely a parameter in a new hypothesis.

4

The discussion in *Scientific Explanation* of the confirmation of statistical hypotheses is much more detailed than the discussion of universal inductions which are confirmed by inductive policies. It is more important, in any case, since if the problem of statistical induction can be solved, we can always consider universal generalisations as merely a special type of statistical induction, in which the parameter assigned to the model has the value 0 or 1. The theory of statistical induction may be taken as the explication of the policy of induction by simple enumeration, upon whose effectiveness our reasonable belief in the effectiveness of every other policy must depend.

Braithwaite points out that the *k*-rule-of-rejection does not provide us with a means of justifying statistical hypotheses. To fail to reject a statistical hypothesis is not necessarily to accept it. There is, however, a theory of statistical inference which appears to be closely related to the *k*-rule-of-rejection: Neyman's theory of confidence intervals.

¹ Braithwaite, op. cit. p. 162

² In the second impression, 1955, and the American Library of Science edition 1956 of Professor Braithwaite's book the following correction was made :—

"empirical tests none of which reject the statement" should read "empirical tests each of which rejects the statement". Ed.

³ Braithwaite, op. cit., p. 166-7. Italics mine.

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Given a probability parameter p for an (α, β) -probability hyperclass, it is possible to calculate two numbers $r_1 = f_1(p)$ and $r_2 = f_2(p)$ such that the probability that an n -fold sample of β 's will possess an α -ratio lying between r_1 and r_2 is equal to $1 - k$, where k is a preselected small number. (There are any number of pairs of numbers satisfying this criterion; but one pair is particularly attractive for the job we want it to do.) It is possible to transform these functions, according to Braithwaite, into two new functions, $p_1 = g_1(r)$ and $p_2 = g_2(r)$ in such a way that, ' . . . whatever be the value p of the probability parameter, the probability is $1 - k$ that this value p is included in the interval $[g_1(r), g_2(r)]$, where r is the α -ratio in a sample of n β -specimens'.¹ I suspect that Neyman would object to having the number $1 - k$ called a probability; he refers to it himself as a 'confidence coefficient'. Like the number k in the k -rule-of-rejection, the number $1 - k$ functions as a description of how probability statements are used; in a given instance, the parameter in question either does or does not fall within the indicated interval, and on Braithwaite's view, as on Neyman's there is no probability involved at all.

If we pass to a higher-level statistical hypothesis, we may suppose, as Neyman seems to suppose, that the method of confidence intervals can be justified by reference to the long run use of the method: if we select a number k and make a great number of inferences of the kind described, then our long-run frequency of success will be roughly $1 - k$. (This, of course, is an empirical truth, not a logical one, if it is true at all.) But with respect to a single experiment the value of k , whether it is 0.80 or 0.90 or 0.99, is of only descriptive significance. It does not constitute, or even form part of, a reason for rejecting or accepting any statistical hypothesis. We therefore have no solution to the problem of enumerative induction: we have no answer to the question, 'Why *ought* we to accept the results of our statistical inferences?' It is simply an empirical fact that we do accept them, and the confidence coefficient merely characterises the pragmatic rule in accordance with which we have acted.

Braithwaite rejects the possibility of distinguishing between the degrees of belief we are entitled to have in different hypotheses. 'The view taken in this book—that the question of the reasonableness of an inductive belief is bound up with that of the validity of the inductive inferences by which it has been or could be derived—thus leaves no room for the comparison of the reasonableness of different inductive

¹ Braithwaite, *op. cit.*, p. 248

beliefs. If the inference is valid, incorporation of belief in its conclusion into a man's rational corpus is justified, and it is reasonable, having the old reasonable belief [in the evidence], to acquire the new one. There is no question of its being more or less reasonable to do so'.¹ 'The reasonable acceptance of a hypothesis is an all-or-none reaction'.²

A confidence coefficient is to have only descriptive significance. But what is its descriptive significance? What is the difference between an all-or-none reaction based on a rule in which the fraction '80/100' appears and an all-or-none reaction based on a rule in which the fraction '99/100' appears? Ordinarily a hypothesis accepted on the basis of a confidence coefficient of 0.99 is considered in a slightly different light than is one accepted on the basis of an inference involving a confidence coefficient of 0.80; the latter is considered not quite as persuasive, as conclusive, as likely, as probable (in ordinary parlance) as the former. It seems natural to suppose that one is entitled to a greater degree of belief in the former than in the latter, or to hold the former with greater tenacity than the latter.

It is often true that we can *act* only on one hypothesis at a time, and that consequently we cannot always be agnostic, no matter how little evidence we have or how evenly balanced the evidence is on both sides of the question. But it is also true that the risk involved in acting on a given hypothesis often depends upon how well-confirmed that hypothesis is; and the more adequately confirmed the hypothesis is, the more confidence we are *rationaly entitled* to feel in acting upon the assumption of its truth. In this sense, it is clearly possible to distinguish between different degrees of rationally justifiable belief. And it is precisely this distinction that different confidence coefficients are intended to express.

Braithwaite confuses a scale of degrees of reasonableness of belief with the quite different scale of degrees of rational belief (degrees of belief of the rational kind), which Keynes and others have had in mind. He thinks that a scale of degrees of reasonableness of belief is a philosopher's myth, but he says, '... although I am unable to recognize differences in the reasonableness of the beliefs which are in my rational corpus, I am able to recognize differences in the *tenacity* with which I hold them'.³ The question of rationality may be brought up in this context as well as in the context of Keynes's discussion of degrees of belief; it is perfectly possible to ask, 'Is it *rational* to hold this belief

¹ Braithwaite, *op. cit.*, p. 357

² *Ibid.*, p. 355

³ *Ibid.*, p. 359

of yours so tenaciously on such little evidence?' This is precisely the equivalent of the question that Keynes and others who have spoken about degrees of rational belief were attempting to consider.

To employ Braithwaite's language, we may say that there is nothing unplausible about supposing that a standard can be formulated which will allow us to distinguish between *rational* and *irrational* degrees of tenacity in given circumstances. On the same evidence, are we not entitled to have a greater degree of belief in the hypothesis that between 40 and 60 per cent. of A's are B's than in the just-acceptable hypothesis that between 45 and 55 per cent. of the A's are B's (or to hold the former hypothesis with greater tenacity than the latter)? And it is tempting to suppose that these rational degrees of tenacity (or rational degrees of belief) are not unrelated, at least where statistical hypotheses are concerned, to the confidence coefficients that appear in the descriptions of what we have done in accepting the hypotheses.

5

There is a further limitation in Braithwaite's treatment of probability and statistical inference which bears heavily on the policy of induction by enumeration. He eschews any mention of randomness in the description of his model, or in his discussion of the relationship between his model and the part of experience which his model is supposed to reflect.

The truth is that the theory of probability to which the balls-in-bags model has to correspond does not require the selections from the bags to be limited in any way. For what corresponds to the conclusion in the deductive system of the theory are propositions . . . about the proportions among the possible selections of those selections which have certain properties. And that in calculating these proportions each of the possible selections must be counted once and once only is a proposition which is true by virtue of the definition of 'proportion'. So it is otiose, and, indeed, misleading to speak of these selections or samples or draws as being 'random', or as being made from a class of 'equally probable' selections or samples or draws, since the equality of treatment which these epithets demand has already been provided by the set-up of the model.¹

To illustrate the difficulties (or oddities) to which this disavowal of the usual randomness requirement might lead, let us consider an

¹ Braithwaite, op. cit., p. 133

example. Let there be a (physical) bag containing ten (physical) balls, of which five are black and have iron cores, and five are white and have lead cores. Suppose that an investigator draws a fairly large number of balls (replacing each before drawing the next), and finds that about two-thirds of the draws result in a black ball. The model which he accepts (on the basis of a given value of k) consists of a large number of (theoretical) bags, each containing two (theoretical) black balls and one (theoretical) white ball. A draw of n balls from the physical bag will correspond to a selection from n of the theoretical bags of the model. The explanation of the probability parameter of two-thirds is that we have supplied our investigator with a (secretly) magnetised grapple, which is just twice as likely to grab an iron-cored ball as a lead-cored one.

Now suppose we get another experimenter to conduct a similar experiment. We give him a non-magnetic grapple (otherwise just like the one used in the first experiment), and he goes through the same procedure as the first man. He will come up with a model consisting of bags each containing two balls, one black and one white; the probability parameter in his hypotheses will be $1/2$.

We might expect the following dialogue to ensue:

A: 'I got a parameter of 0.5; is that what you got?'

B: '0.5? Impossible! I got one of 0.67.'

A: 'There's something very odd going on here.'

B: 'You said it! Say, let's trade grapples and see what happens; maybe that has something to do with it.'

But on Braithwaite's interpretation of statistical hypotheses, there is nothing odd about the situation at all. Where randomness is not involved, neither is bias. In each case it is perfectly plausible to assume that there is a statistical hypothesis, and since each hypothesis only asserts something about the class of samples *drawn* from the parent population, and nothing about the parent population itself, there is no reason why both A's hypothesis and B's hypothesis should not be accepted. The dialogue must run:

A: 'I got a parameter of 0.5; is that about what you got?'

B: 'No, I got a parameter of 0.67.'

A: 'Splendid, old man. We only expected to come up with one hypothesis, and now it looks like we've got two. We can both publish!'

If we are to accept both hypotheses, we must distinguish between them; the most plausible way is to mention the method of drawing

samples to which the hypothesis applies. Our investigators, not knowing about the magnetic grapple, will only be able to say that one hypothesis applies to 'A's method' and the other applies to 'B's method'. From this example, it is clear that the hypothesis asserts only that *the samples drawn from a parent population by a certain method* correspond to a selection from a probability hyperclass which has a certain parameter. A statistical hypothesis tells us something about the samples drawn by a given method and from a given population, but not about the composition of the population itself.

Now suppose the experimenter with the magnetic grapple draws more balls at a time; if he draws three balls at a time the probability parameter of his statistical hypothesis will be 0.65 rather than 0.67; if he draws ten balls at a time, before replacing them, the parameter will be 0.5. It is clear that if the population is finite, we should include the size of the sample as well as the method of drawing it in the statement of the statistical hypothesis.

The value of the parameter p in the statistical hypothesis which will never be rejected by the k -rule-of-rejection, if there is such a value, is dependent on (a) the method of drawing the samples, and (b) the size of the samples which are drawn between replacements, if the population is finite, as well as (c) the constitution of the population being sampled.

6

Braithwaite's attempt to justify the inductive policy of induction by simple enumeration was not satisfactory; one argument established that if it were in fact a valid policy, then we could reasonably believe in it, but did not establish that it was in fact a valid policy, and hence did not establish that we could in fact reasonably believe in it. The other argument was so weak that it could be used as well to justify the prediction of the future from tea-leaves as to justify the policy of induction by simple enumeration. From this failure, it follows that we cannot (yet) justify the other inductive policies by reference to the policy of induction by simple enumeration.

We may also, I think, drop the reference to inductive policies altogether without altering the picture; the empirical element which is essential to the statement of the effectiveness of an inductive policy can be expressed as an ordinary empirical hypothesis, and we may of course use well-tested empirical hypotheses in arguments leading to

new inductive conclusions. The basic difficulty seems to lie in the justification of induction by simple enumeration.

As Braithwaite points out, the policy of induction by simple enumeration can be considered merely a special case of the general policy of making statistical inductions. If we can show that the argument from a sample to a population is *reasonable*, we will have shown that induction by simple enumeration is reasonable; and if induction by simple enumeration is reasonable, then it can be used to confirm hypotheses and theories which in turn may be employed in future arguments.

Braithwaite's treatment of probability and of statistical hypotheses does not seem to be adequate to the justification of induction by enumeration, however. The mysterious k which appears both in the elucidation of the meaning of statistical hypotheses and in the procedure for testing statistical hypotheses, represents merely a part of the description of how probability statements (statistical hypotheses) are *used*; it does not tell us why they are or ought to be used that way, and it provides no *justification* for accepting a hypothesis which will pass the confirmation test we have chosen for it. In particular, on Braithwaite's theory, there seems to be no reason for choosing a test employing one confidence/coefficient rather than another. If a 'reason' is offered, it must be construed as another statistical hypothesis, subject, like the original hypothesis, to confirmation or refutation.

Braithwaite's conception of beliefs as either reasonable or unreasonable, regardless of the degree of tenacity with which they are held, while it is in conformity with his cavalier treatment of confidence coefficients, does not seem plausible. While it may be 'reasonable' to believe the statistical hypothesis that very close to nine-tenths of the electrons fired at a target in a particular experiment will hit it, it is not so clearly 'reasonable' to believe that a particular fall of a die will result in some side other than the one-spot, and although we may be forced to act on one assumption or the other, it is difficult to know whether on a toss of a coin belief in the occurrence of heads or in the occurrence of tails would be 'reasonable'. We are often concerned with general statistical hypotheses (e.g. that about half of all coin tosses result in heads) simply because they provide grounds for determining the appropriate (rational) degrees of partial belief in particular instances.

Finally, the attempt to do without randomness results in some unusual limitations in the scope of statistical hypotheses: (1) the hypothesis applies only to a specific method M employed in obtaining

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the samples on the basis of which it is confirmed or rejected; (2) the hypothesis only applies to a specific sample size (between replacements) unless the parent population is infinite; and (3) the hypothesis can only refer to the composition of the samples drawn (of the given size and by the given method) and does not generally give us any non-hypothetical information about the composition of the parent population. In order to pass from knowledge of the composition of the samples to knowledge of the composition of the parent population, some statement about the randomness of the sample must be employed.¹

While it may be possible to accept these restrictions, most statistical assertions do not conform to them. In most branches of science it is felt that the data obtained from samples give some information about the parent population from which the samples were drawn. On Braithwaite's interpretation there is no need to be concerned about randomness or bias; the more discordant results that are obtained, the more valid statistical hypotheses there are. But statisticians are often very much concerned with randomness, and they do not always seem to treat an assertion about randomness as an empirical hypothesis to be confirmed like any other. In many cases it is clear that any attempt to confirm such an hypothesis would be absurd. There is a considerable discrepancy between the theory proposed by Braithwaite and the practice of statisticians.

This is not the place for me to present an alternative to Braithwaite's theory of probability.² I have indicated that Braithwaite's theory of induction is unsatisfactory when it comes to the fundamental form of induction—the confirmation of statistical hypotheses. I think that the difficulties in his treatment of the confirmation of statistical hypotheses stem from an inadequacy of his theory of probability, and in particular from a model interpretation of probability which does not take into account the serious problems of interpreting randomness. Given a definition of randomness which allows us to relate statistical probabilities to degrees of belief, we can overcome these difficulties. It is precisely in this relation that a solution to the problem of induction must be sought.

¹ This need *not* be an empirical statement to the effect that the method draws each possible sample equally often—it may be a merely logical statement to the effect that we have no grounds (logical or empirical) in our rational corpora for supposing the method to be biased.

² I have in process a rather lengthy treatise, *Probability and the Logic of Rational Belief*, which does present an alternative treatment of probability.

H. E. KYBURG

The general scheme for the justification of induction presented in *Scientific Explanation* still seems appealing and valid. If we can establish the reasonableness of making statistical inductions (or a framework for evaluating the degree of belief in a statistical hypothesis which is warranted by the evidence for the hypothesis), then we can proceed in accordance with Braithwaite's scheme. Braithwaite's major contribution to the problem of induction consists in the fact that he focuses his attention on the central problem of confirming statistical hypotheses where his predecessors had erroneously concentrated on the problem of confirming universal generalisations.

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DISCUSSIONS

ON MR ROY HARROD'S NEW ARGUMENT FOR INDUCTION

IN Mr Roy Harrod's book *Foundations of Inductive Logic*, ably reviewed by Dr Bronowski in this *Journal*, there occurs an argument which is both completely new and crucial for the book. I have talked about this argument with a number of people, knowledgeable in this field, and they all reacted in the same way. They were all convinced that the argument was invalid; they all could give good and sufficient reasons why it *had to be* invalid; but they could not pin-point the mistake. They rightly remarked that Mr Harrod expresses himself very vaguely at the decisive places; but I was not convinced that this remark was a perfect substitute for pin-pointing the mistake. For Mr Harrod's crucial argument is completely new; it is a bold argument and shows much ingenuity. I felt, for this reason, that an attempt to show, not so much why Mr Harrod's argument *must be* invalid, but rather why it *is* invalid, may perhaps be not quite superfluous.

Mr Harrod tries to establish a proposition which may be formulated as follows.

If we travel in a vehicle through a desert which we have entered at zero minutes, then there is always, at m minutes a probability equal to 0.99 of not reaching the end of the desert at $m + (m/99)$ minutes; or more generally, there is a probability equal to $x/(x+1)$ of our not reaching the end at $m + (m/x)$ minutes.

Now since this holds for every number m , we can say that there is always a very high probability, $p = x/(x+1)$, of something that has been going on for some period of time to go on for just a little longer; which does not sound unreasonable.

This, I think, is the crucial proposition Mr Harrod wishes to prove. His argument is quite simple, and might be expressed, a little more precisely, in the *language of betting*, as follows.

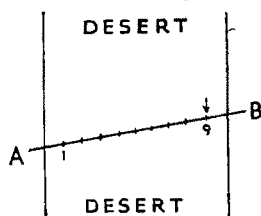
Assume that two gamblers, Peter and Paul, agree on the following betting contract. It is assumed that at any minute m from the moment of entry into the desert, a bet is made according to which Peter owes Paul $x/(x+1)$ pennies if the end of the desert is reached within the next m/x minutes. If the end is not reached within this period of time, then Paul owes Peter $1/(x+1)$ pennies. After reaching the end of the desert, no further bet is made, and the debts are settled.

It can easily be shown that, upon the assumption that the units chosen—the 'minutes'—are small as compared with the total time of crossing the desert, this contract is a *fair betting contract*, in the sense that, upon conclusion of the contract and settling of the debts, it will be found that neither of the two gamblers owes the other gambler anything. Now this means that the odds of x to 1 in favour of not reaching the end during the next period of time were *fair odds*; which means, further, that the probability p of not reaching the end of the desert within that period is $x/(x+1)$.

That the betting contract is fair may be shown, following Mr Harrod, by the following diagram (in which we assume that we proceed with constant velocity—an assumption which need not be challenged).

K. R. POPPER

The path from A to B is our way through the desert. We freely choose a number x —say, $x = 9$, for the purpose of this diagram—and divide the distance from A to B into $x + 1$ parts (i.e. 10 parts). The arrow in our diagram points to the beginning of the last (10th) part. It is clear that Peter's bet that we shall not reach B within the period of time m/x will be won by Peter anywhere before the arrow, and thus in nine cases out of ten.



Accordingly, there is at every minute m a probability of $p = x/(x + 1)$ of not reaching the end of the desert ; which seems to prove Mr Harrod's proposition.

This, I believe, is Mr Harrod's argument, restated in terms of a gambling contract.

A slight variant would be the following. Instead of contracting (as in our 'first contract') to gamble at every minute (or some other unit of time), the parties may agree on a 'second contract' according to which they gamble only once (or twice . . .), at an instant of time m determined by drawing a ball (or two balls . . .) from an urn, before entering the desert. If the ball shows the number m , then, according to the contract, the game is played at m minutes, if the desert is not at an end at m minutes ; and should it so happen that the end of the desert is reached before these m minutes have elapsed, then the contract lapses, and no game is played.

If the game is played, then the odds are determined as before : there will be fair odds of x to 1, or a probability of $p = x/(x + 1)$, in favour of not reaching the end of the desert within the next m/x minutes.

Now these calculations can all be easily checked, and they are correct. What, therefore, is wrong with Mr Harrod's argument ?

My reply is this. Simple as this argument is, it is sufficiently complicated to make its verbal formulation not too easy. Accordingly, it is not quite easy to say in two words *what the event is* whose probability p we have calculated as $p = x/(x + 1)$. It is through this verbal complexity that the mistake creeps in.

Mr Harrod suggests that if I travel through the desert, knowing that I entered it m minutes ago, but not knowing when it will end, then there will be a probability of $x/(x + 1)$ —say of 99/100—of the event of 'not reaching the end of the desert during the next $m/99$ minutes'.

But this interpretation of the meaning of p can be easily shown to be mistaken, with the help of the following simple proposition :

The gambling contracts described remain fair, even if either Peter or Paul or both know exactly when the end of the desert will be reached (for example, at 60 minutes).

That this is so is clear from the generality of the arrangement : from the fact that the contracts are *generally* fair—that all the conditions assuming fairness are incorporated in the contract and that lack of knowledge was not one of the stipulated conditions.

ON ROY HARROD'S NEW ARGUMENT FOR INDUCTION

It follows from this that the probability p which Mr Harrod *correctly* calculates *cannot* be a probability that may be interpreted as the degree of Peter's or Paul's imperfect knowledge.

This is all that is needed to answer Mr Harrod. Thus the complete mathematical generality of his model establishes the fact that he misinterpreted his result.

If the question is asked, What is p ? What is the event whose probability equals p ? then the answer will be a little different according to the two contracts.

Under the *first contract*, p is nothing but the proportion of the number of games which Peter is bound to win to the total number of games to be played under the contract.

Under the *second contract*, p is the classical probability of Peter winning his gamble: it may be large because it is unlikely that we shall draw a number placing the bet just immediately before the end of the desert. Mr Harrod expresses this by saying that it is unlikely that we shall find ourselves just at the edge of the desert. This is correct: it is unlikely that we shall find ourselves, *by accident*, close to any *given spot*; for example, within a unit from the end of the desert—or *from its beginning*. But this has nothing whatever to do with our knowledge, or our lack of knowledge, of where that spot is; and it therefore must not be interpreted as a measure of the completeness or incompleteness of that knowledge.

It may be helpful to discuss an objection to my analysis. My argument, it may rightly be said, depends upon Peter and Paul *entering into unalterable contract*. Clearly, once they have entered into an unalterable contract, new knowledge—such as information concerning the moment at which the end of the desert will be reached—cannot make any difference. But if we allow Peter, or Paul, to *cancel* the contract—for example, the first contract—then any superior knowledge possessed by the one or by the other will enable him to win.

My reply is this. If we permit Peter to cancel the first contract (exactly analogous remarks hold for the second contract), then this is *sufficient* to make it an *unfair* betting contract. For Peter can now, *without any further knowledge*, improve his chances of winning; for example, by cancelling the contract *always* after the first bet. Since we have to assume in any case that the units of time—that is to say, the distances between the bets—will be small as compared with the total travelling time across the desert, the policy of cancelling *after* the first bet would mean that Peter can win his bet regularly. (If Paul has the right of cancelling, he would, accordingly, always cancel the first bet itself; so that there would never be a game if both can cancel.) We thus see that it is essential to the fairness of the betting contract that neither Peter nor Paul has the right to cancel it. Accordingly, Peter's (or Paul's) foreknowledge of the end of the journey through the desert cannot influence the game at all (provided the game was fair); and consequently, the probability p must not be interpreted as a probability that may be ascribed to an uncertain hypothesis concerning the future arrival at the end of the desert. Thus p is completely different, in this respect, to the probability, say, of betting on heads or tails in an ordinary game; for here the probability remains $\frac{1}{2}$, even if we allow either party to cancel the bet beforehand; and in this way, foreknowledge of the result possessed by one party would increase the probability of a win by this party from $\frac{1}{2}$ to 1. (Of course, any properly calculated probability is always that of a *fair* gamble.)

In conclusion, a simple paradox may be presented that appears to be fatal for Mr Harrod's inductivist interpretation of his probabilities.

We slowly travel along a row of equally spaced telegraph posts. Mr Harrod's argument allows us to say (by choosing an appropriate x) that there is a probability as great as we like that this type of row will continue for a distance of at least $1/x$ of the stretch it has so far continued; from which we will be able to conclude that there is a very high probability of encountering the next telegraph posts within, say, one half of a second.

But Mr Harrod's argument is applicable to any kind of stretch or gap, allowing us to predict, with arbitrarily high probability, that the stretch or gap in question will continue a little longer. It is thus applicable to the gaps between any two telegraph poles, and it may yield a very high probability that we shall *not* encounter the next telegraph pole within one half of a second. Yet these two probabilities contradict each other.

Our own solution is obvious: the odds refer to entirely different gambling contracts (to different *relative* probabilities). Yet the paradox establishes that Mr Harrod's always applicable method of calculation cannot possibly be interpreted as referring to inductive probabilities. It is not his mathematics that is at fault but his interpretation; and to avoid misinterpretation of this kind, one of the best general methods is to state one's argument in terms of an appropriate gambling contract.

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'PHILOSOPHICAL' AND 'SCIENTIFIC' SENSE-DATA

IN a recent review of my book in this journal Mr Tucker draws a distinction between 'philosophical' and 'scientific' sense-data on the grounds that they are different kinds of *entity*. This distinction seems to be in error. Sense-data, as I have defined them, are empirical entities and philosophers such as Russell, Moore, Broad, and Price are clearly spending much of their time talking about 'scientific' sense-data. Moore¹ defines sense-data in a way very similar to mine with reference to after-images and the others clearly use the term to refer to the empirical entities that they find available to introspection in their visual fields (see e.g. Broad²). Having noticed and described these empirical entities, it is then possible to make epistemological analyses such as 'Only statements about sense-data are certainly true' or 'Our knowledge about physical objects is based on an acquaintance with sense-data', etc. But it is completely misleading to claim that the 'philosophical' sense-data and the 'scientific' sense-data are different kinds of *entity*. When we say 'Our knowledge about objects is based on our acquaintance with sense-data' we are talking epistemologically about 'scientific' sense-data and not about something else called 'philosophical' sense-data. Sense-

¹ G. E. Moore, 'Visual sense-data', *British Philosophy in the Mid-Century* (C. A. Mace, Ed.), London, 1957

² C. D. Broad, *Religion, Philosophy and Psychological Research*, London, 1953, pp. 32 and 37

MULTIDIMENSIONAL TIME

data are empirical entities which have certain kinds of relations, to minds and to physical objects, that give rise to epistemological arguments about certainty, etc. But the sense-data mentioned in these epistemological arguments,¹ or even defined epistemologically,¹ are not some kind of shadowy entity over and above the empirical sense-data. Of course we can also discuss the *concept* of sense-data, but the only wits we have to keep about us here are those that we normally use to distinguish between entities, our concepts about these entities and the words that we use to express these concepts and to denote these entities. When Broad and Moore ask what is the relation R between sense-data and physical objects they are not discussing the relation between our concepts of sense-data and our concepts of physical objects, but they are discussing the relation between sense-data and physical objects. To say that 'philosophical sense-datum theories are about epistemological certainty' implying that they are not about empirical sense-data is like saying 'A jury's verdict is about certainty' implying that it is not about murder. So far from my being deceived by puns, Tucker is the victim of a category mistake that one would have supposed that so close a follower of Professor Ryle could have avoided more easily.

J. R. SMYTHIES

MULTIDIMENSIONAL TIME

I HAVE the following comments to offer upon Dr Mario Bunge's note in which ² he refers to earlier papers by myself on the subject of two-dimensional time.

(i) I have not tried to import *psychological* implications into mathematical expressions occurring in physical theories. I have only attempted to indicate some logical consequences of postulating a parallelism between certain psychological and physical facts.

(ii) I have not suggested that there is anything 'mysterious' or 'unreal' in connection with complex variables, or with the pure imaginary parts of complex variables. On the contrary, I have been at pains to emphasise that, if one does introduce a second time dimension, as the pure imaginary part of a complex time variable, this pure imaginary time must be given full physical significance.

(iii) The essential point of the second sentence of the last paragraph of Bunge's note, if I have understood it, is that where complex variables enter into the mathematical expression of physical theories, both the 'real' and 'pure imaginary' parts of such variables not only *can*, but *must*, have a physical 'correlate' (i.e. an identifiable referent in the physical world). That both parts of a complex variable *can* have full physical significance in this sense is, of course, well known to anyone who is acquainted with the elementary theory of electricity, where a particularly simple example is to be found in the so-called 'impedance' of a circuit. Not only do both the real and pure imaginary parts of the complex quantity called 'impedance' have physical

¹ E.g. 'whatever we can be certainly said to perceive', that is in terms of incorrigibility: this still *refers*, however, to empirical sense-data.

² This *Journal*, 1958, 9, 39

correlates, but both can be separately measured. However, at the Quantum-mechanical level of physical phenomena the matter is not so simple because, in general, as has been emphasised by Dirac 'No meaning can be attached to the measurement of a complex observable'. Dirac points out¹ that 'It would not do to measure separately the real and pure imaginary parts because this would mean two measurements which in general would interfere with one another'.

(iv) Bunge omits all mention of the troublesome considerations which physicists themselves call 'reality conditions'. Indeed he seems to overlook 'reality conditions' altogether when he claims that 'complex quantities entering physical theories are—provided they do not play a barely intermediary syntactical rôle—just pairs of real quantities having each a physical correlate'. This statement is *not* true in the case of those physical theories in which 'reality conditions' are imposed in order to deprive certain complex quantities of physical significance, without thereby relegating them to a 'barely intermediary syntactical rôle'. A well-known example of such reality conditions is Dirac's postulate, restricting the representatives of physical observables to those Hermitian operators which satisfy his expansion theorem, with the corollary that an arbitrary eigen state of a physical observable is dependent upon the eigen states of Hermitian operators. In claiming that Dirac's reality conditions do not come within the scope of Bunge's remarks I admit that I am not clear as to the exact meaning of his phrase 'a barely intermediary syntactical rôle', in connection with the question of the physical interpretation of mathematical expressions. However it would seem that a clear example of a mathematical quantity which plays a 'barely intermediary syntactical rôle' is Dirac's improper δ -function. This δ -function never appears, in a final result except as a factor in an integrand. Consequently, as Dirac says,² 'The use of [such] improper functions is . . . a convenient notation enabling us to express in a concise form certain fundamental formulas [*sic*] which we could, if necessary, rewrite in a rigorous form, but only in a cumbersome way in which the parallelism with the case of discrete eigen values would be obscured'. Thus Dirac's improper δ -function does clearly play a 'barely intermediary syntactical rôle'. But it is equally clear that this rôle is different from that played by the complex, non-diagonal, elements of matrices representing 'observables' in quantum mechanics: these latter quantities could not be eliminated merely by rewriting the mathematical expressions 'in a cumbersome way'. Thus complex quantities, subject to 'reality conditions', can in general neither be regarded as 'Playing a barely intermediary syntactical rôle', nor as just 'pairs of real quantities each having a physical correlate'.

Despite these criticisms of Dr Bunge's note I find it interesting to learn from him that theoretical physicists are now finding it convenient to use the language of two-dimensional time to express theories of 'sub-nuclear phenomena' (i.e. phenomena whose explanation involves the internal structure of the 'elementary particles' of matter). Theories of sub-nuclear structure are certainly required to bring order into the jungle of observed facts about the 'elementary particles' currently postulated by theoretical physics. For example, it is very probable that a satisfactory theory of the magnetic moment of the neutron will require a hypothesis ascribing to the

¹ *Quantum Mechanics*, 2nd edn., 1935, p. 29 n.

² *Ibid.*, p. 74

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neutron an internal structure comprising an equal number of spinning electrical point charges of opposite sign (chirality), whose motions are not symmetrical with respect to the centroid of the system. (Such lack of symmetry in its internal structure might well be the clue to the problem of the instability of the free neutron.) A model of this kind can be constructed, using Bunge's complex time-operator, in such a way that spinning charges of opposite electrical sign (chirality) have components of momentum with opposite sign with respect to the pure imaginary time. Thus it becomes possible for pairs of charges of opposite sign (chirality) to cohabit the same world-line in an electrically neutral region of ordinary 4-dimensional space-time, while having different 5th dimensional components. So we get a picture of pair production in which 'anti-particles' (i.e. particles of opposite chirality) are treated as particles of the same nature with opposite components in the 5th dimension. Prior to pair production anti-particles can be conceived as initially cohabiting the same world-line in ordinary (electrically neutral) 4-dimensional space-time, but as having opposite 5th dimensional components with respect to the imaginary time dimension, which cancel each other. An initially neutral system of this kind can break down into two chirally opposite particles, following different world-lines in 4-dimensional space-time, whenever the system is subject to a perturbation of sufficient energy. When, for example, a positron and an electron are produced from the interaction of a gamma ray photon with a magnetic field in a Wilson Cloud Chamber, what happens, according to this picture, is the splitting of an electrically neutral world-line into two different loci, which have components of momentum of opposite sign with respect to the 5th dimension. Similarly, the annihilation of two anti-particles on collision is depicted as the conflation of two loci, each of which has, initially, a 5th dimensional component of momentum of opposite sign with respect to the pure imaginary part of the complex time, to yield a world-line with no resultant component of momentum with respect to the imaginary time dimension.

A satisfactory theory of pair production and pair annihilation is only one of the advantages furnished by the adoption of a five dimensional theory, employing a complex time, in which both the real and pure imaginary parts are given full physical significance.

H. A. C. DOBBS

THE PARADOXES OF CONFIRMATION

AN INTERESTING article¹ by Mr J. W. N. Watkins has given rise very recently to an exchange of papers between him and Professor Hempel.² Both Watkins's original article and the further papers are chiefly concerned with the problem of providing a general criterion for distinguishing empirical from non-empirical statements. One of the topics discussed at some length is the problem raised by Hempel's 'paradoxes

¹ Watkins, 'Between Analytic and Empirical', *Philosophy*, 1957, 33, 112-131

² Hempel, 'Empirical Statements and Falsifiability', *Philosophy*, 1958, 34, 342-8; Watkins, 'A Rejoinder to Professor Hempel's Reply', *Philosophy*, 1958, 34, 349-55.

of confirmation'.¹ In this note I shall first try to show that Watkins is mistaken in claiming that the paradoxes provide a conclusive argument in support of his position and then I shall attempt to explain how the paradoxes arise.

I

In order to give an account of the paradoxes, one cannot do better than quote Hempel's own summary (Hempel, 1958, p. 344).

It seems reasonable to consider an object, say b , as confirming a hypothesis of universal conditional form, such as ' $(x)[\text{Raven}(x) \supset \text{Black}(x)]$ ', if b satisfies the antecedent and the consequent of the conditional, i.e. if b is a raven and is black. It also seems reasonable to agree that an object confirming a given hypothesis confirms as well any statement logically equivalent to it. Now, the hypothesis just mentioned is equivalent to the statements ' $(x)[\sim \text{Black}(x) \supset \sim \text{Raven}(x)]$ ' and ' $(x)[(\text{Black}(x) \vee \sim \text{Black}(x)) \supset (\sim \text{Raven}(x) \vee \text{Black}(x))]$ ', the former of which is confirmed by anything that is neither black nor a raven, the latter by anything that is not a raven or is black. Hence any object of either of these types must be qualified as confirming the hypothesis that all ravens are black.

Hempel accepts this conclusion and in his original article argues that it only appears paradoxical because we do not normally consider hypotheses in isolation but only in the context of an existing body of presuppositions. 'If we are careful to avoid this reference to additional knowledge . . . and if we formulate the question as to the confirming character of the evidence in a manner adequate to the concept of confirmation as used in this paper . . . the paradoxes vanish' (Hempel, 1958, p. 20).

Watkins, on the other hand, draws the conclusion that we should abandon the notion of confirmation through observation of instances and should instead follow Popper in interpreting 'the confirmation of a testable hypothesis in terms of persistent and unsuccessful attempts to falsify it. . . . A confirmed hypothesis is a hypothesis which has "proved its mettle" by withstanding severe tests' (Watkins, 1957, p. 117). And in his latest paper he writes (p. 351):

On a Popperian theory of confirmation this hypothesis is confirmed by an observation-report of a black raven, not because this reports an instance of the hypothesis—a white swan is also an instance of it—but because it reports a satisfactory test of the hypothesis: a raven has been examined unsuccessfully for non-blackness. Statements about non-ravens which do not report tests of our hypothesis cannot confirm it.

He also points out that it seems very strange to accept a theory of confirmation according to which a hypothesis can be confirmed equally by two such flatly contradictory statements as ' a is a cat and not black' and ' a is no cat and black'.

One must admit that Watkins is in general right in stressing the importance of Popper's falsifiability criterion. As applied to scientific theories it is clearly fundamental. But it is not so obvious that the criterion is applicable in exactly the same way and with exactly the same force when one is considering simple generalisations such as 'All ravens are black', especially when these are taken in an artificial isolation. One possible difficulty is that Watkins, having rejected the notion of confirmation

¹ See Hempel, 'Studies in the Logic of Confirmation', *Mind*, 1945, 54, 1-26 and 97-121

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by observation of instances at the first level, may find himself re-introducing it at the next. For he speaks of confirmation of a hypothesis 'in terms of persistent and unsuccessful attempts to falsify it'. But this seems to suggest an investigator arguing, 'Many individual efforts to falsify this hypothesis have failed; therefore it is highly probable that all subsequent efforts will fail'. It seems that each of the unsuccessful efforts will count as confirmation of the second level hypothesis which states that the first level hypothesis will never be falsified. This objection does not, of course, bear directly on the paradoxes; in fact, if one tried to consider the second-level hypothesis in isolation the paradoxes would arise again at this next stage. What it does seem to show is that the notion of instantial confirmation is just as basic as that of falsifiability, since the rationale of the latter cannot be explained without using the former notion—the alternative of explaining the apparent confirmation of the second-level hypothesis in terms of unsuccessful attempts to falsify a third-level hypothesis being clearly unacceptable since it would start one on an infinite regress.

In the present context, a far more important objection to Watkins is that his suggestion does not in fact help us to avoid the paradoxes. He implies that they only arise if we talk of confirmation, and that if instead we spoke only of attempts to falsify the paradoxes would disappear. But in order to show this he will have to explain more fully the notion of 'attempts to falsify' or 'tests of the hypothesis'. For here also we must presumably accept an equivalence condition such as: whatever counts as an attempt to falsify (or as a test of) a hypothesis p must count as an attempt to falsify (or a test of) any hypothesis which is logically equivalent to p . But if we accept this condition then whatever counts as an attempt to falsify 'All non-black things are non-ravens' will count as an attempt to falsify 'All ravens are black'. The presupposition of Hempel's discussion is that we must consider only the hypothesis and not introduce tacit reference to any of our existing knowledge about ravens, black objects, etc. If we accept this limitation then there in general seems no reason why the finding of black shoes (there *might* have been a non-black raven in the closet) or of white swans (there *might* have been some non-black ravens in the swannery) should not count as unsuccessful attempts to falsify ' $(x)[\sim \text{Raven}(x) \vee \text{Black}(x)]$ ' and therefore as unsuccessful attempts to falsify 'All ravens are black'.

The first conclusion I would draw from this is that Watkins does not succeed in solving or dissolving the paradoxes. The second, that the notion of confirmation through unsuccessful attempts at falsification will need explanation and modification if it is to be applied to simple generalisations; for in this type of case it is by no means obvious what is to count as a test of the hypothesis.

2

If, then, the paradoxes occur with equal force if one accepts Watkins's position, one is compelled to look for some other explanation of them. The general form that such an explanation must have was outlined by Hempel in his original article, where he writes, 'In the seemingly paradoxical cases of confirmation we are often not judging the relation of the given evidence, E alone to the hypothesis H . . . ; instead, we tacitly introduce a comparison of H with a body of evidence which consists of E in conjunction with an additional amount of information which we happen to have at our disposal' (Hempel, 1945, p. 20). But it is important to see exactly what is this additional amount of information which we usually have or presuppose that we have.

It is hardly enough just to accept the fact that the paradoxes disappear if we limit our evidence; we also want an account which will explain why the paradoxes arise in the first place.

Hempel himself suggests that the extra evidence which we tacitly presuppose is the non-emptiness of the subject class—in his example we presuppose that there are ravens. He writes (Hempel, 1945, p. 17):

... if someone feels that objects of types c and d (c = black non-ravens, d = non-black non-ravens) are irrelevant rather than confirming ... then this may mean that he is consciously or unconsciously construing the latter as having existential import; and this kind of understanding of general hypotheses is in fact very common. In this case the 'paradox' may be removed by pointing out that an adequate symbolization of the intended meaning requires the adjunction of an existential clause.

But this surely is wrong. For suppose we do interpret 'All ravens are black' as ' $(x) [\text{Raven}(x) \supset \text{Black}(x)]$ ' ($(\exists x) [\text{Raven}(x)]$), then it is clear that observation of non-ravens cannot be said to confirm both parts of this hypothesis. But observation of one raven will *conclusively* confirm the second part of it. Thus after one raven has been observed we will only be interested in collecting evidence in support of the first part of the hypothesis and since, as we have seen, this is equivalent to ' $(x) [\sim \text{Raven}(x) \vee \text{Black}(x)]$ ', we shall again apparently have to admit white swans etc. as confirmatory evidence. Thus the paradox is not removed.

So we must look elsewhere to find what additional information is usually presupposed. An example of a situation in a closed group will help us. Suppose we are considering the hypothesis that all philosophy post-graduate students have I.Q.'s of more than 120 and let us suppose also that we already know independently that such students (ϕ) form 1 per cent of the total number of students and that there are exactly 20 per cent of students with I.Q.'s of more than 120 (ψ). Then if there were no correlation whatsoever between being ϕ and being ψ (i.e. being a philosophy post-graduate student and having an I.Q. of more than 120) we might expect the following distribution.

(a) $\phi \cdot \psi$	(b) $p \cdot \sim \psi$	(c) $\sim \phi \cdot \psi$	(d) $\sim \phi \cdot \sim \psi$
0.2 per cent	0.8 per cent	19.8 per cent	79.2 per cent

But if it were the case that all ϕ students were ψ , we should expect

1 per cent	—	19 per cent	80 per cent
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Now if we simplify the situation still further so that we only have to choose between the hypothesis that all p objects ϕ objects are ψ and the hypothesis that there is no correlation at all, then it will be clear that a instances will provide more confirmation than d instances.¹ Two different but converging arguments can be used to show

¹ The problem of assigning a probability value to a hypothesis on the basis of evidence from a sample is a difficult and controversial one, and many would agree with Kneale (*Probability and Induction*, pp. 211 f.) when he argues that it is impossible to assign exact numerical values to the probability of hypotheses. But if we are not prepared to accept a cardinal scale for degrees of confirmation, we must surely admit the possibility of saying in some cases that one parcel of evidence provides more confirmation for a hypothesis than another—three black ravens clearly provides more evidence for 'All ravens are black' than one black raven.

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this. Firstly one can argue that if one finds say 10 a instances this would constitute very strong evidence for the hypothesis that all ϕ objects are ψ because the chances of finding 10 a instances and no b instances among 10 ϕ objects would, if there were no correlation, be very small—in my example $(\frac{1}{8})^{10}$. On the other hand, the chances of finding 10 d instances and no b instances among 10 $\sim\psi$ objects would be considerable— $(\frac{9}{10})^{10}$ —even if there were no correlation. Secondly one can point to the fact that if the hypothesis is true the total number of a instances in the population will be 5 times greater than it would be if there were no correlation. Thus if the percentage of a instances in a random sample of the population is around 1 per cent or greater than 1 per cent it will make it more likely that the hypothesis that all ϕ objects are ψ is true. On the other hand, the percentage of d instances in this example will be almost the same whether or not the hypothesis is correct, and thus it will be impossible to draw any argument for or against the hypothesis solely from the proportion of d objects in a sample of the population. The same is true of c instances though, if anything, a higher percentage of these in a random sample would tend to be disconfirmatory.

Of these two arguments, the first is clearly more convincing and also nearer to the way in which we usually reason—and we are attempting to discover what are the assumptions made by us in our everyday arguments, the assumptions which make Hempel's argument appear so paradoxical. For the second argument would only be cogent if we made some more or less exact numerical estimates of the different frequencies involved, whereas the figures involved in the first argument are of such different orders of magnitude that any calculation is superfluous. It is true, of course, that the situation will be far more complicated if we do not just have to choose between the hypothesis that all ϕ objects are ψ and the hypothesis that there is no correlation. But even if we allow for the possibility of there being different degrees of positive correlation, it is clear that a instances will, in the absence of any extra information, provide better confirmation for the hypothesis that all ϕ objects are ψ than d instances.

On the other hand, it is comparatively easy to imagine a situation in which this would not be true. Suppose we are visiting an American campus where we know that 80 per cent of the male students are undergraduates (ϕ) and that 90 per cent of the male students have short hair (ψ). We then consider the hypothesis that all male undergraduates have short hair—that all ϕ objects are ψ . In this case the corresponding figures would be

(a) $\phi \cdot \psi$	(b) $\phi \cdot \sim\psi$	(c) $\sim\phi \cdot \psi$	(d) $\sim\phi \cdot \sim\psi$
72 per cent	8 per cent	18 per cent	2 per cent
if there is no correlation; while they could be			
80 per cent	—	10 per cent	10 per cent
if all ϕ objects are ψ			

In such a situation d instances would provide better confirmation than a instances—that is, we should be more profitably employed looking for long-haired graduate students than for short-haired undergraduates.

A similar example would be the hypothesis 'All deciduous trees are less than 200 feet in height'. Here again we would look at the trees which are more than 200 feet in height to see whether or not any of them are deciduous, rather than make records of the various deciduous trees less than 200 feet high.

In general, if the probability of an object being ϕ is x and the probability of it being ψ is y ($y > x$ since we assume that it is possible for all ϕ objects to be ψ) the distribution if there were no correlation would be

$$\begin{array}{cccc} (a) & \phi \cdot \psi & (b) & \phi \cdot \sim\psi & (c) & \sim\phi \cdot \psi & (d) & \sim\phi \cdot \sim\psi \\ & xy & & x(1-y) & & y(1-x) & & (1-x)(1-y) \end{array}$$

and if the hypothesis were true

$$\begin{array}{cccc} x & \text{---} & y-x & 1-y \end{array}$$

The first line of argument would be as follows. If there were in fact no correlation between being ϕ and being ψ , then the chances of a random collection of n ϕ objects all being ψ (i.e. all a instances and no b instances) would be $(y)^n$, the chances that a random collection of $\sim\psi$ objects will all be $\sim\phi$ (all d instances and no b instances) would be $(1-x)^n$. Thus if and only if $y < 1-x$ will a instances provide better confirmation than d instances.

The second, and less cogent, argument gives the same condition. According to this the confirmatory power of different types of instance will depend on the relative expectations of finding such instances at all, according to whether there is no correlation or a complete correlation. Thus, if there is no correlation the probability of an a instance occurring is xy ; if the hypothesis is true the probability is increased to x , that is increased by a factor of $\frac{1}{y}$. The similar increase in the probability of

d instances will be by a factor of $\frac{1}{1-x}$. Thus the relative increase will be greater for a instances than d instances if and only if $y < 1-x$.

We are now in a position to draw some conclusions. The first is that a necessary condition for a instances to provide better confirmation than d instances is that the probability of an object being ϕ shall be less than the probability of it being $\sim\psi$ — e.g. the probability of something being a raven less than the probability of it being not-black.¹ It is obvious that this condition is almost always satisfied in the hypotheses expressed in an ordinary language; for almost always the subject and predicate classes will be small compared with the total universe of discourse. In situations where the condition does not hold we would almost certainly have simple predicate phrases to refer to the classes complementary to those of x and y so that we should in practice be more likely to formulate the hypothesis in the form 'All $\sim\psi$ objects are $\sim\phi$ ' e.g. 'all long-haired men are graduate students' or 'all the trees over 200 feet high are coniferous'. So I would suggest that the paradoxes arise because we always tacitly presuppose that this condition holds. Hempel's concept of confirmation seems outrageous because he is considering the ideal situation in which our minds though well-stocked with concepts are *tabulae rasae* as far as facts are concerned, the sort of situation in which we know how to recognise ravens and black objects, but in which we do not know that the probability of finding a raven is very considerably less than the probability of finding an object (animal, vegetable, or mineral) which is not black.

¹ This conclusion has already been derived as a theorem in the development of an axiomatised theory of confirmation by Dr Janina Hosiasson-Lindenbaum, *The Journal of Symbolic Logic*, 1940, 5, 133-148

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Secondly we might draw an additional argument against Watkins's position as actually stated by him. For in one of the passages about the raven hypothesis he says that statements about non-ravens cannot confirm it because they do not report tests of the hypothesis (Watkins, 1958, 351). This would suggest that in the exceptional cases, such as the case of the missing long-haired undergraduate or the deciduous trees, Watkins would have to say that observation of long-haired graduate students or coniferous trees more than 200 feet high would not count as reports of tests of the respective hypotheses and so would not count as confirmatory. And this, we have seen, would be wrong.

But further reflection shows that perhaps in one way Watkins is right. For the important feature of such arguments is not the mere occurrence of a instances (or, in the exceptional case, d instances) but their occurrence in conjunction with the absence of b instances. If we only knew that so many a instances occurred in a collection of objects otherwise unspecified, we should be forced to rely on what I called above the second and less cogent argument from the expected frequency of a instances in the total population. This line of argument would involve some complicated calculations and would be impossible unless we had accurate knowledge of the actual probabilities of an object being ϕ , or of being ψ . In fact the argument on which we surely normally rely starts from the fact that unless there is a connection between being ϕ and being ψ , it is extremely unlikely that we should get a collection of n ϕ objects all of which are also ψ . In the exceptional case we would argue similarly from the improbability of getting n $\sim\psi$ objects all of which are also $\sim\phi$.

One puzzle remains—the status of c instances. Why are c instances not confirmatory? For surely one could argue in a similar way from the improbability of getting in a collection of objects so many c instances and no b instances. The answer is two-fold. Firstly it will always be the case that either a or d instances will provide better evidence than c instances. Secondly that in practice we are very unlikely to consider them; for it would involve searching for objects that were either b or c instances, i.e. either $\phi \cdot \sim\psi$ or $\sim\phi \cdot \psi$, that is, for instance, looking for objects which were either non-black ravens or black non-ravens. It would always be easier to search for ϕ objects, whether ψ or $\sim\psi$ (a and b instances) or for $\sim\psi$ objects, whether ϕ or $\sim\phi$ (b and d instances).

What I have been trying to do in the second half of this note is to explain *why* we normally regard a instances as alone providing confirmation. The problem is here that of elucidating our usual methods of argument in cases where we speak of evidence confirming a hypothesis, cases in which we already assume some prior knowledge; it is not the problem of providing a better method of argument based on a new formally defined concept of confirmation and applying this method to hypotheses considered in complete isolation. My conclusion was firstly that in such cases our method of argument is from the improbability of getting so many a instances and no b instances if there were no connection, and secondly that in such arguments we presuppose that the probability of an object being ϕ is much less than the probability of it being $\sim\psi$ —a presupposition which is almost always justified.

H. G. ALEXANDER

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KOYRÉ ON THE HISTORY OF COSMOLOGY

THIS is a straightforward story, lucidly told.¹ It is the story of the development of the finite universe of the middle ages into an infinite one. We see how the finite universe slowly disintegrates and the infinite universe of Newton ultimately establishes itself.

Koyré's straightforward story is not a story without a moral. In the introduction to this volume Koyré refers to the views which animate his presentation. His basic idea is that the rise of modern science is bound up with a change of world-view, with a change, that is, of metaphysical outlook. Now this is surely a thesis which flatly contradicts the central thesis of positivism, or anti-metaphysics, or empiricism. (In Koyré's terminology 'empiricism' signifies the view that scientific assertions are proved by experience.) But Koyré presents his thesis with no direct reference to this as his arch-enemy. Positivism, Koyré asserts, is a very ancient doctrine; he sees in it an enemy who is an old acquaintance too. Perhaps it is his familiarity with this old enemy which leads Koyré merely to point an occasional shaft in its direction, as when he expresses his feelings by adding the word 'disastrous' to a remark concerning the present divorce between science and philosophy. Yet his main thesis is just this: 'The conception of the infinity of the universe is, of course, a purely metaphysical doctrine that may well—as it did—form a basis for empirical science; it can never be based on empiricism' (p. 58). This is the moral of the present volume—even though it is stated explicitly only when it serves as a part of a specific historical explanation. And the power of the present volume lies in the obvious fact, that in spite of the author's conscious attempt to avoid side issues as much as possible, he is *forced*, time and again, to discuss metaphysical and even theological arguments, without which his story could not be presented adequately or even intelligibly.

I do not see how a positivist can possibly answer Koyré's historical argument convincingly. Indeed, I think that most positivists who have studied the history of Renaissance physics—including Duhem—have had to concede something, however grudgingly, to the kind of view which Koyré adopts. Yet Koyré's work shows how very much they have to concede. Of course, one can still admit refuting instances, and declare that the refuted theory holds for all other cases. In science this procedure is

¹ A. Koyré, *From the Closed World to the Infinite Universe*, Johns Hopkins Press, Baltimore (London: Oxford University Press). Pp. x + 304. 40s.

considered very unsatisfactory, and a theory which is rendered too shifting and *ad hoc* by this procedure will sooner or later be rejected. In philosophy it is customary to dismiss the refuting instance with an old and lame excuse, as Stuart Hampshire did quite recently, claiming that the metaphysical basis of modern science is merely a peculiarity of its infancy. The metaphysical systems of the Renaissance, Hampshire admits, are 'evidently designed to prescribe the outlines of a new physical science'. Yet they were concocted only as an antidote to Aristotle; in our own day 'the construction of metaphysical systems of any kind has come to seem finally useless and impossible'. Again, 'we no longer have any need for armchair programmes of science'.¹ What admittedly worked in the past is declared to be impossible now. But Hampshire's argument is very unconvincing (if for no other reason than that we *may* need an antidote to the antidote to Aristotle). His argument was already stated at least a century ago, for it was reported and answered by Maxwell. 'It has been asserted' Maxwell tells us,² 'that metaphysical speculation is a thing of the past and that physical science has extirpated it. The discussion . . . , however, does not appear to be in danger of coming to an end in our time, and the exercise of speculation continues as fascinating to every fresh mind as it was in the days of Thales.' In other words, science is always in its infancy; in each period we start a *new* discussion concerning *new* problems, and the starting point is speculative or metaphysical. Thus, the 'useless and impossible' discussion is still going strong.

I have tried to present and defend the methodological views implicit in Koyré's book, believing that I may thus be able to show the topical interest of what is primarily a historical volume. I believe that even the most convinced positivist, if he will be critical enough to accept the challenge and read the present volume carefully, will find doubts uneasily stirring in his mind. Having emphasised its philosophical significance I shall now offer some further, mainly historical, additional remarks concerning Koyré's

¹ Stuart Hampshire, *Spinoza*, Penguin edition, 1951, Chapter VI, pp. 211-212. Cf. Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, Part II, Chapter 2. Hampshire presents this view tentatively, and even reports later some vague objections to it. But he ultimately commits himself to this view (p. 226). One need hardly say that this view contradicts the linguistic form of positivism (according to which metaphysical 'utterances' can never have any meaning). Yet after a sensible presentation of Spinoza's metaphysics and after admitting its relevance to physics, Hampshire remains true to linguistic positivism, considering metaphysics as meaningless and divorced from science (at least from now on).

² James Clerk Maxwell, *Scientific Papers*, Cambridge, 1890, Vol. II, p. 225. For Maxwell's marvellous 'armchair program' see his letter to Faraday of 1857 published in Rollo Appleyard's *A Tribute to Faraday*, 1931, p. 111-13. Faraday's reply to this letter is quoted below, p. 238, n. 2. For his despair of the programme see op. cit., p. 125 and introduction to his *Matter and Motion*.

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subject-matter. I wish to emphasise, however, that these remarks are merely intended as marginal notes to a fascinating book. The main point of the general remarks is more to give the reader a general idea of the background than to criticise Koyré's brilliant historical synthesis.

* * *

1. Koyré's discussion of the priority concerning the view of the infinite universe may be correct. But it is not supported by any argument against Jammer's attribution of the idea to Rabbi Hasdi Crescas (*Concepts of Space*, pp. 79-80). This is a point at issue, since Jammer contends—rightly or wrongly—that Crescas influenced Telesio who, in his turn, influenced Giordano Bruno to whom Koyré attributes priority (p. 39). But it is preferable not to discuss priority of the general idea—which is ancient anyhow—but the question: who raised the *problem* by criticising accepted views? From this angle I tend to believe that Crescas's criticism of Aristotle was of paramount importance, while, no doubt, his positive theory (being pre-Copernican) seems less imaginative and revolutionary than that of Bruno.

2. Koyré suggests (p. 56) that Gilbert hesitated over whether to accept Copernicanism. The basis for Koyré's suggestion is the fact that, while arguing for the existence of the earth's diurnal motion (or rotation round its axis) Gilbert 'consistently avoids saying,' (p. 286) explicitly whether or not he admits the earth's annual motion (or rotation round the sun). Koyré asserts his view diffidently, and even mentions a weaker alternative to it, namely, that dealing with terrestrial phenomena (magnetism) Gilbert was uninterested in the problem of the annual motion of the earth. But both these hypotheses are untenable.

Gilbert does not consider magnetism to be a terrestrial phenomenon but rather a cosmic feature which he identifies with form (in the Platonic sense), with light (in the wider neo-Platonic sense), and with force (which is his own new metaphysical notion). Thus he considers magnetism to be the cause of all celestial action.¹

Moreover, Gilbert does commit himself to Copernicanism, even though he deliberately avoids speaking explicitly about the annual motion of the earth. This is clearly seen in the following passage (from Chapter III, Book IV, of *On Magnets*): 'III. *That the Earth moves circularly.* I pass over the reasons (*sic*) for the Earth's remaining motions, for at present the only question is concerning its diurnal motion.' In another passage (*ibid.*, Chapter IV), not only the Copernican theory but also the Copernican Sun-worship are manifest: 'the Earth is moved by her own primary form and natural desire, for the conservation, perfection, and ordering of its parts, towards things more excellent: and this is more likely than that the fixed stars, those luminous globes, as well as the Wanderers, and the most glorious

¹ For all this see last pages of *On Magnets*.

and divine Sun . . . should circulate aimlessly round the Earth. . . .¹ Or, in other words, the theory of the Earth's diurnal motion is more plausible since it follows from Copernicanism. (By the way, he speaks very highly of Copernicus as the reviver of astronomy.)

Furthermore, Gilbert speaks of the sun as the chief cause of motion, and he explains the different length of the different planetary years by assuming a fixed proportion between each planet's year and its distance from the sun. These two theories make Copernicanism an inescapable conclusion even though in this place his evasion is most peculiar. (As Professor Koyré kindly pointed out to me this law of linear relation between the planetary year and the planetary distance was four years previously published by Kepler in his *Mysterium Cosmographicum*; he had thought of it before he hit on his famous 'third law' and soon rejected it as inadequate. Rough linearity was already indicated by Copernicus.)

Ultimately, the only point of Gilbert's theory of gravity (which is, again, an elaboration of Copernicus's view) is that it enables the earth to move in space (and it is for this reason that Galileo accepted it from Gilbert). The theory is, briefly, that gravitation is merely the tendency of the parts to unite with the whole, so that the whole itself has no weight and is free to move, not only round its axis but also in space. He concludes his chapter on gravity (Book VI Chapter 5) thus: 'Therefore weight neither hinders the diurnal revolution, nor influences either direction or continuance in position.'

These are my reasons for rejecting the tentative suggestions which Koyré does not propound with much conviction anyway. Koyré has raised here a fascinating problem which was hardly noticed before.² It is indeed a strange fact that careful reading of Gilbert convinced even a judicious scholar like S. P. Thompson,³ that he definitely asserted something which, on closer inspection, will be found to be at most only an implication or even merely an allusion. Although, like Thompson I regard Gilbert as a Copernican, I have no solution to Koyré's problem about why Gilbert very carefully refrains from a frank assertion of the existence of the earth's annual motion. (It should be remembered that Gilbert had no religious scruples here: the greater heresy was to admit the earth's diurnal motion, as Joshua attributed diurnal motion to the sun.)

¹ Dreyer, to whom Koyré fully refers, merely asserts that Gilbert was a follower of Brahe; which, though palpably false and superficial, might, perhaps, pass as a kind of indication of the present problem. But Dreyer was the first to notice Gilbert's evasion.

² S. P. Thompson, *Notes on De Magnete*, privately printed, London, 1901, especially p. 62. See also his *Gilbert of Colchester*, London 1901, p. 31, where he speaks of Gilbert's 'frank acceptance of the astronomical doctrines of Copernicus'. This assertion is refuted by Koyré.

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However, it is quite clear that Gilbert's work is designed to assist the establishment of Copernicanism. There may have been two reasons for his embarking on his magnetic research. The one is that the magnetic theory accepted at his time (that of Paracelsus and Cardan) assumed magnetism to be caused by some celestial bodies, which Gilbert tried to test, evidently with an eye to its possible relevance to the astronomical controversy, and which he later refuted. The other is this. The main obstacle to Copernicanism was Aristotle's theory of natural motion, which asserted that natural motion is either circular or in a straight line towards the centre. Since Copernicus deprived the earth of its central position it was necessary to find independent arguments against Aristotle's theories of motion and of gravity—a problem dealt with mainly by Gilbert and by Galileo. Indeed Gilbert asserts that he considered the natural motion of the magnetic needle (circulating towards a fixed direction) as a refutation of Aristotle.

So much for Gilbert's initial purpose. As to his main achievement, he believed that his magnetic theory gave new arguments in support of the earth's diurnal rotation, and this may well be why he did not wish to assert more. This idea gains slight support from a work of Barlowe, a friend of Gilbert who defended his views on magnets though not on astronomy:¹

But concerning his (Gilbert's) Sixth Booke entreating the motion of the earth, I thinke there is no man living farther from beleeuing it, than my selfe, being nothing at all perswaded thereunto, by the reasons of other men, which he alledgeth, and as little or lesse (if it were possible) by those his inventions, endeavouring to proue the motion of the earth by the earthes *Magneticall* force and vertue.

The 'reasons of other men which' Gilbert 'alledgeth' are mainly those of Copernicus, of course; though Barlowe is vague on this question. It is remarkable that even Barlowe, Gilbert's only disciple, was unwilling to admit Gilbert's strongest astronomical point, and this may partly explain Gilbert's reluctance to go further into the matter. I do not know how much one's pupils' dogmatism may render one reticent as to one's revolutionary views, but it seems that these things do happen. In a reply to an open-minded letter of Maxwell, Faraday admitted² that he would not express in public certain revolutionary ideas of his. Gilbert was

¹ W. Barlowe, *Magneticall Advertisements*, London, 1616, 2nd edn. 1618, preface to reader.

² H. Bence-Jones, *Life and Letters of Faraday*, London, 1870, Vol. II, p. 392: 'The idea that we may possibly have to connect *repulsion* with the lines of gravitation-force (which is going far beyond anything my mind would venture on at present, except in private cogitation) shows how far we *may* depart from' Newton's views on gravitation. (See note 2, p. 235, above for the reference of Maxwell's letter.)

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even less fortunate than Faraday, of course, the scientific tradition not having been developed as yet. However, I do not know how my conjecture can be checked, and I consider Koyré's problem still open.

3. Gilbert's further contribution towards Copernicanism is the new solution he suggested towards the problem of infinity. Koyré does not discuss the views of infinity in connection with Euclidean geometry (which is a pity), though he discusses this aspect in his *Galilean Studies*. For it is clear¹ that one of the main reasons for the acceptance of infinity is the Renaissance acceptance of the geometrical outlook, or, as Bruno calls it, Pythagoreanism. This is in fact Plato's revised version of Pythagoreanism (intended to solve the problem of the irrationals), in which geometry, or figure, takes the place of arithmetics, or number, and proportion takes the place of ratio. (This is admirably discussed in Popper's works²—for the first time, I think.) The interest of Kepler, Gilbert, and Galileo, in genuinely geometrical models (i.e. ones which contain proportions) is evidence, if evidence is necessary, for their Platonism; Copernicus's Platonism (or rather neo-Platonism) is expressed in the view that the sun symbolises the Idea of the Good and should therefore be the centre of the universe. (See Burt's classical *The Metaphysical Foundations of Modern Physical Science*, London, 1925.) Now Euclidean geometry assumes an infinite universe, but the infinity of the universe is incompatible with the idea of a centre to the universe. This is why Copernicus and Kepler are finitists, while Bruno and Gilbert are infinitists. Gilbert's solution to the problem of centrality in an infinite universe is this: the sun is not in the central spatial position, but central in another way, being the primary mover, or the cause of motion. This 'magnetic' (i.e. dynamic) theory contradicts Plato's idea (in the *Timaeus*) that all (inanimate) motion is caused by impact or push, which Gilbert quotes disapprovingly. Thus Gilbert's solution is not quite acceptable to a full-fledged Platonist. (Indeed, Plato was a finitist; yet Euclid who carried out a part of Plato's geometrical programme, was forced into a different position.)

However, Gilbert was the person who made the Copernicans conscious of the fact that their acceptance of Euclidean geometry introduces problems which demand a solution. It is perhaps a pity that Koyré glosses over

¹ Cf. 'All the movements of the loadstone are in accord with the geometry and form of the Earth and are strictly controlled thereby, as will later be proved by experiments and diagrams.' Or 'Hence the magnetic nature is proper to the Earth and is implanted in all its real parts according to a primal and admirable proportion.' (Pp. 67 and 105 in Mottale's translation of *On Magnets*.)

² See K. R. Popper, *The Open Society*, 3rd edn., London, 1957, Vol. I, Chapter 6, n. 9 and *Addendum*, as well as his 'The Nature of Philosophical Problems', this *Journal*, 1952, 3, pp. 134-154. As to my remarks below concerning Euclid, see there pp. 147-148 and 152.

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Gilbert's geometrism and dynamism, accepting the widespread myth of Gilbert's animism. This myth is partly due to the animism of the ancient Pythagorean school, partly due to mechanist prejudices (mechanism being the theory that all acceleration is caused by push, which Galileo, Descartes, and Boyle, made the authoritative view then), partly due to the later tendency to emphasise the difference between Gilbert and Newton, and partly due to a still later confusion caused by a failure to realise that by endowing magnets with souls Gilbert merely endowed them with (force and) harmony, following the Pythagorean-Platonist theory which viewed the soul (as self moving and) as harmony. The viewing of the world as harmonious was shared by Oersted (the author of the wonderful *The Soul of Nature*, London, 1848) and Einstein, among other modern philosophers. The view of the soul as a harmony is also not quite an outdated view, be it true or false. Admittedly Gilbert is more anthropomorphist than, say, Einstein, but this is due to the general tendency towards ever increasing abstractness.

4. Koyré asserts that Kepler rejected the view of the infinite universe on empiricist grounds and that Kepler even claimed that the idea of infinity is meaningless. It seems to me, however, that even if Koyré interprets correctly the texts he relies on, he takes them much more seriously than Kepler had intended, considering that Koyré himself does not claim that Kepler was an empiricist. Kepler's position is quite clear. 'My opinion that there are unseen comets in the sky is disputed by many' he writes.¹ 'One asks how could I know this. But I do not say that I know it. . . . You believe that one should see them if they exist. I deny this.' And in many places he praises, like Galileo, the method of hypothesis, as the opposite of the theory which bases scientific views on observations. It seems to me that Koyré slightly underrates Kepler as a methodologist, although I admit that there is a kernel of truth in his exposition, and even an important one which enables us to accept and even strengthen his presentation.

Kepler believed (like Galileo) that the world is knowable and that therefore false hypotheses should be rather easily refutable within a short time so that truth will soon be established.

This optimistic theory of the 'nearness' of the truth² lends some force to Kepler's central argument against infinity. The argument, which Koyré discusses in detail, is very roughly this. If the world is infinite then the two following propositions must hold. (a) The stars must be well ordered, say, like atoms in an infinite crystal. (b) The world looks pretty much the same from any position. And Kepler shows that the conjunction of these two

¹ Carola Baumgarten, *Johannes Kepler : Life and Letters*, London, 1952, p. 78.

² For this theory see K. R. Popper, 'Die öffentliche Meinung im Lichte der Grundsätze des Liberalismus', *Ordo*, 8, 1956.

statements is refuted, because from certain stars other stars appear about three times larger than the apparent size of the sun as viewed from the earth. Kepler's argument is beautiful and most imaginative, yet the connection between the infinity of the universe and the conjunction of the two propositions (a) and (b) which he tried to refute is not clear unless one remembers his optimist view of the world as being sufficiently simple. (Incidentally the second proposition, that the world must look pretty much the same from any position, is strikingly reminiscent of the modern cosmological principle. The difference between Kepler and the moderns is this. Kepler's physics is mainly geometry and he therefore prescribes similarity of geometrical initial conditions, i.e. position and magnitude, while the modern cosmologists postulate a similarity of initial conditions relevant to modern theories.)

5. Koyré is puzzled by Newton's use of the variability of gravitational forces to support his refusal to consider gravity as an essential property (p. 175). An essential property is constant. Thus, since mass is constant, inertia which depends on mass only is essential, while gravitation cannot be essential since it is variable, as it depends on distance which constantly changes. Koyré, however, thinks that from Newton's own point of view gravitation is just as constant as inertia. 'The *weight* of a body', he agrees, is indeed variable, 'but the attractive force of the earth . . . is constant, and, just as in the case of inertia, proportional to its mass, and it is as such that it appears in the famous inverse square formula of universal gravitation' he asserts. I am afraid I am unable to follow this argument unless it is this: Newton's *gravitational mass* is constant, while merely the intensity of its effect—the *gravitational force*—decreases as the effect of a source of light of a constant intensity decreases with the increase of the distance from it. This is the argument of Roger Cotes (Preface to second edition of *Principia*). But Cotes's argument is evasive since it is not the propensity to gravitate but the actual gravitational force which is under discussion, and here Cotes's analogy breaks down, since the light particle is a *thing* while the force is the *action* of one thing on another. As Professor Popper puts it, Newton could not view gravitation as essential since it is *relational*, while an essential property can belong only to the one substance whose essence it determines. The variability of gravitation proves its relational character.

6. One of the most interesting aspects of the book is the discussion of the relations between natural philosophy and rational theology. Here the discussion of the physico-theological (teleological), proof of the existence of God. The physico-theological 'proof' is this: the existing order of the universe is an argument in favour of the theory that the universe was ordered by a divine architect. Yet the historical significance of the 'proof' lies in the fact that it was inverted. The theory of a divine

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orderer was used to support or undermine different theories concerning the order of the universe itself. For the divine architect had a divine purpose in ordering his world, and the (purposeful) order is such that becomes the architect who is so truly divine. This inversion of the argument not only explains why the physico-theological proof was indeed used, by Bentley and others, as physico-teleological—to use Koyré's label—but it explains indeed, as Koyré shows in great detail, the penetration of theology into scientific discussions.

The general view in the mid-seventeenth century was that theology and philosophy should be kept apart. This view was accepted on the authority of Galileo, Bacon, and Boyle. Boyle (who introduced the label 'physico-theological') was one of the most ardent separators of religion and philosophy. He held an all-embracing metaphysical dualism with reason, physico-theology, philosophy, and justice on one side, and on the other, emotion, religion, fine arts, and mercy. He concluded from this that the religious arguments from miracles have no rational value whatever, and have merely a sentimental appeal. He saw in the physico-theology (or rational theology, or natural theology) the basis of natural religion (or rational religion) and philosophising—experimenting included—as the ritual of that religion. Physico-theology served as an encouragement for philosophic activity.¹ It also served as a safeguard to assure peaceful co-existence between philosophy and religion (by excluding scientific materialism which might incite hostility towards religion and also by prohibiting religious disputes in philosophical circles).

But Boyle's Cartesian universe which seemed at first so well ordered had certain defects. The watch demanded not only a watch maker (to use Boyle's metaphor) but also a constant rewinding and mending (to use Koyré's metaphor). The argument about whether it is becoming for the architect to make a watch which is in need of rewinding put the problem of conservation of energy in the centre of the controversy. This controversy probably started with Bentley's *Boyle Lectures*, which were intended as a reassessment of Boyle's philosophy, and which were never published in their original form, because of the famous correspondence between Bentley and Newton, which forced Newton, and later Bentley, to change their views. Bentley forced Newton to think about the theological implication of his own theory, especially in the light of the problem of the validity or invalidity of the mechanical philosophy. For Bentley's starting point in his Boyle's lectures was this mechanical philosophy, and he wanted Newton to corroborate both mechanism and its (alleged) theological implication. Challenged, Newton worked at his problem, but he ultimately

¹ Boyle's so-called 'theological' writings, from his first publication (*Seraphick Love*) onwards, were not theological, but outspokenly propaganda for the new experimental philosophy.

gave up hope of finding a solution. In order to understand the *instantaneous action at a distance* of the stars upon one another, he called in the omnipresence of God. (I owe this point to Professor Popper.) Thus God was introduced as the frame of the world machine, if not just a cog in it, much to Leibniz's dismay. Attacking Newton's heresy (while referring to Boyle's piety and to Boyle's mechanism), he alleged that it is unbecoming for God to create a watch which cannot maintain itself. This argument gave rise to the Leibniz-Clarke *Correspondence*, which, as Koyré shows, brought the problem of the conservation of energy (or *vis viva*) into the foreground. The surprising fact was that Newton's theory turned out to conform to the Leibnizian law of conservation of energy, which, in its turn, was factually corroborated. The watch needed no rewinding after all. Physico-theology could be relegated to the wings, if not ignored altogether.

The story of Laplace's answer to Napoleon 'Sir, I do not need this hypothesis' (of the existence of God) is very famous. Koyré has succeeded in putting it in its historical perspective and in giving it a new interest by interpreting it literally rather than as a sarcastic remark. It marked the end of an interesting intellectual co-operation between theology and philosophy, and the starting point of the nineteenth-century scientific materialism, a movement which dismissed this previous co-operation with theology as a minor incident. It was soon entirely forgotten, to be rediscovered only in our own time.

7. Physico-theology is only one of the historical roots of this co-operation. The other was the discussion between More and Descartes. Descartes, the Copernican, refused to ascribe infinity to space (for reasons discussed above, under point 2). He declared that God and only God is truly infinite. More, the infinitist, overcame this theological difficulty when he identified God and space, thus anticipating Newton's theory of space as God's sensorium. I think both suggestions, Descartes's as well as More's, are rather evasive. Descartes's idea of an 'indefinite' universe is vague, and is used as an evasion of a genuine problem. More's criticism of Descartes, based on the interpretation of 'infinite' as 'non-finite', seems to me unanswerable. If Descartes's 'indefinite' implies merely that the size of the universe is unknowable, then the question of the truth or falsity of Euclidean geometry is undecidable, which is counter to Descartes's view of Euclidean geometry. If the universe is non-finite, but of a different category of non-finitude than the one to which God's non-finitude belongs, then Descartes had committed himself to the view that the sun is not in the centre of the universe while, in fact, he did not wish to commit himself on this issue. (As Koyré argues, he held his tongue in his cheek, believing in Copernicanism while pretending to conform to Catholicism.) However, his excuse for his evasion—his refusal to ascribe infinity to anything but God—had to be answered, and was answered by More's identification of God and space.

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I do not find the identification very impressive, in spite of all the Cabbalistic arguments which More marshalled in defence of this view. Yet even this unsatisfactory debate between Descartes and More was pregnant with interesting ideas which were pursued in the dispute between Newton and Leibniz mentioned above.

8. The end of the relationship between natural philosophy and natural theology was primarily due to the success of Newton's natural philosophy, and also to the decline of theology in general, as Koyré points out. The decline in theology was in turn due largely to the wave of Biblical criticism and to criticism of the pretensions of theology which had already started in the seventeenth century. As Mr J. W. N. Watkins kindly pointed out to me a good deal of this Biblical criticism was historically based on a theory of the 'nearness' of (religious) truth analogous to the theory of the 'nearness' of (natural) truth mentioned above, under point 4. The most impressive criticism of theology was perhaps Kant's refutation of the traditional proofs of the existence of God. Koyré rightly mentions (p. 180) Kant's appreciation of the physico-theological proof, but it should be added, perhaps, that Kant appreciated it only as an argument, not as a logical proof. Kant himself was satisfied with a deity whose existence is undemonstrable, but for others this was a dethronement of God, or, as Heine put it rather poetically, the death of God.

Kant's reintroduction of God in his system of ethics, is a much discussed point. But he also introduced God, in a fashion, already in his philosophy of science. This seems to me to be his Boylean approach, common to many thinkers of the period of self-improvement or Enlightenment, that is to say, the view of philosophising as a natural worship of God, or as a moral duty. Natural philosophy was an active 'intellectual love of God' in Spinoza's idiom. Today 'secular' ethics had freed itself of theology, and so has philosophising. Even Einstein, who considers the 'cosmic religious feelings' as essential for scientific research, refuses to take any interest in any personal deity whatsoever.¹ The intellectual interest in God seems to be a thing of the past.

* * *

I have made the above notes in an attempt to give to the reader a general idea of the book. I have left out many interesting points in an attempt at an outline of some general themes in the work. The apparent form of the work is that of a presentation of various views of various thinkers concerning the question at hand, the question of the size of the universe. Yet, of course, two or three themes become rather outstanding in the

¹ A. Einstein, *The World As I See It*, 'Science and Religion'; also *Out Of My Late Years*, 'Science and Religion'.

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discussion of that question. I have mentioned Copernicanism and geometrisation and the problems they involve, the mechanical philosophy and its amendments which call in theological considerations. All these points are discussed by Koyré with great lucidity. The greatness of critical speculative thinking is in working out positions in detail, and Koyré does justice to his heroes by showing, in each case, how their views on a specific question (is the world finite ?) are linked with their other views ; whether they present together a coherent picture of the world ; and how these coherent world-pictures were criticised.

I wish to repeat that my remarks are merely intended as marginal notes to a fascinating book. It is much easier to sum up all one's comments and criticism of a good book than all that one wishes to say in its favour, or even all that one might say about the new ground gained in it. I should only mention that, among other merits, its presentation of Descartes's natural philosophy, of Kepler's arguments, of Raphson's and Bentley's views, and of the development of the views after Newton, are, to my knowledge, entirely new, and truly fascinating. The work is a model of clarity, precision, and documentation. In the best French tradition it makes very easy reading yet at no sacrifice of profundity. With very few authors Koyré might, in good conscience, repeat Kepler's assertions¹:

‘I have therefore wished to remind the reader that everyone likes his own way best, that most people get heated in their discussions and that, in my opinion, humour is by far the best seasoning of any debate. Others, in philosophising, try to impress people with too much grave solemnity but very often give a ridiculous impression this way quite against their will. I believe that I am made for mitigating the troubles and bothers of research work by expressing good humour in my style.’

J. AGASSI

The Philosophy of Science. By P. Henry van Laer, in collaboration with Henry J. Koren.

Editions E. Nauwelaerts, Louvain, 1956. Pp. 159. \$3.75.

In this book and its projected successor, Dr van Laer, who is professor of philosophy at Leyden, has set himself a very ambitious programme. In the present volume he wishes to deal with the problems common to all the sciences; in the second, to characterise the several groups of sciences. By ‘science’ he does not mean the natural sciences alone; he includes not only the social and psychological sciences but also philosophy and theology. He lists the common characteristics of these studies: logical order, a definite

¹ Carola Baumgarten, *op. cit.*, p. 83.

field, interest in universal statements, and so on. He derives a list of their 'constitutive elements or aspects' (p. 14): They all use abstraction; their statements have a character of necessity; they have each their characteristic area of study and definite point of view, their starting-point and presuppositions, their proper methods; they all have a structure of hypothesis and theory, and depend on the logic of proof. This analysis constitutes the author's programme, and he proceeds to devote a chapter to each point. He wishes to give a general account of the terrain, before drawing a detailed map of the region of systematised knowledge.

Such an attempt was worth making, for the time is ripe for comparison of the methods of different fields of study, in relation to the different problems to which they are relevant. The programme, therefore, is a promising one. Unfortunately the working-out is less satisfactory, especially as far as natural science is concerned. Dr van Laer has clearly a good grasp of physics and of traditional logic, and a wide range of interests; but he passes altogether too lightly over the special problems of the natural sciences. In these sciences we are faced, as Mr Kneale put it in his *Probability and Induction*, with 'untransparent necessitations'; the reasons why things should be thus and not otherwise are opaque to us. Dr van Laer assumes too easily that we have insight into the nature of things, and this leads him to underestimate the difficulties involved in assessing the status of scientific laws and theories. He thinks that we know by direct insight that nature is deterministic and must therefore be regular in operation. Such an easy solution of the question of induction—which according to Dr van Laer is a problem only for empiricists, not for realists—is the more surprising in that Dr van Laer has had experience of physical research, having worked with Professor Keesom on the properties of superconductors at low temperatures.

The book invites comparison with another recently-issued work, written within much the same tradition, which also in its way aims at a map of knowledge: Jacques Maritain's *Degrees of Knowledge*. Maritain makes less use of formal logic, and his book, which is much longer, is in some ways more diffuse; yet it seems to the reviewer to give a more delicate and more accurate picture of the range of human knowledge. Again, Maritain is no physicist, but he shows a surer touch in his treatment of the physical sciences and the revisions they require of the traditional treatment of abstraction. Neither work takes adequate account of English and American work in the last forty years on the logic of science.

The English of this edition is in general clear and readable, but a number of oddities need to be corrected: *crystallic* for *crystalline* (p. 45), *realistic* for *realist* (p. 131), *genial* for *of genius* (p. 120), *facilities* for *faculties* (p. 149), and the terms *ideal* and *experiential* applied to sciences; while John Stuart Mill is made curiously unfamiliar by the omission of his first name.

E. F. CALDIN

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In his Introduction, however, Nagel confesses himself dissatisfied with a purely instrumental view, but unable at present to find a satisfactory *via media* between the extreme standpoints on this issue, and he remarks that such very general problems cannot be solved wholesale, but only after careful analysis of particular examples. The hope that he will proceed to such analysis is in the main disappointed, for most of the subsequent articles remain on too general a level. This criticism does not apply however to an article on 'A Formalization of Functionalism', for here Nagel gives an interesting and detailed comparison between a formalism describing functional or goal-directed systems as found in biology, and the data and concepts of the social sciences. This is an excellent example of the philosophy of science at work on a particular type of theory, and one hopes to see more from Professor Nagel in the same vein.

MARY B. HESSE

Meaning and Necessity, A Study in Semantics and Modal Logic.

By Rudolf Carnap.

Second edition, enlarged. The University of Chicago Press, Chicago, Illinois, 1956. Pp. x + 258. • 37s. 6d.

THE present edition contains the text of the first edition in its original form, and a supplement consisting of five previously published essays, which to a large extent were prompted by discussions and criticism of the author's semantical method. The subject-matter of the essays is reflected in their titles, which are as follows: (a) 'Empiricism, Semantics, and Ontology', first published in the *Revue Internationale de philosophie*, 1950, 4, 20-40; (b) 'Meaning Postulates', *Philosophical Studies*, 1952, 3, 65-73; (c) 'On Belief Sentences' in *Philosophy and Analysis*, ed. M. Macdonald, Oxford, 1954; (d) 'Meaning and Synonymy in Natural Languages', *Philosophical Studies*, 1955, 7, 33-47; and (e) 'On Some Concepts of Pragmatics', *Philosophical Studies*, 1955, 6, 89-91. All these essays are related to various sections of the main body of the book. For the most part they clarify the author's position, and in some cases they modify his earlier views. In this respect, 'Empiricism, Semantics, and Ontology' seems to be of particular interest as in its light many a student of semantics may have to revise his interpretation of Carnap's theories. It is, therefore, to this essay and its bearing on the foundations of semantics that the reviewer proposes to confine his observations.

The purpose of the essay, which is related to the discussion in sections 6 and 10 of the main body of the book, is to clarify the issue between those semanticists who admit abstract entities such as properties, relations, propositions, etc., and those who oppose the procedure. In order to understand

more clearly the nature of the problem Carnap suggests that we should distinguish two kinds of question concerning the existence of entities: first, we have *internal questions*, i.e. questions of the existence of entities within a *linguistic framework*, secondly there may be *external questions* referring to the existence or reality of *the system of entities as a whole*. This distinction appears to constitute the substance of Carnap's argument against the nominalistic scruples in the Quine-Goodman style, and against the "Fido"—Fido' criticism extended by Ryle. According to Carnap the acceptance of a kind of entities in the *internal sense* amounts to introducing (a) constants which are said to be names of the entities in question, (b) a general term or predicate permitting us to say of any particular entity belonging to the kind of entities under consideration that it does belong to this kind, and (c) a corresponding type of variables of which the entities are said to be the values, and for which the constants can be substituted. Now, the questions of the existence of any entities in the *external sense* are rejected by Carnap as pseudo-questions, and any thesis which gives expression to a belief in the reality of such entities is branded as a metaphysical pseudo-statement.

It would appear from what Carnap says in his essay that whenever he talks about introducing or accepting abstract entities, he no more believes in the reality of these entities than we believe in the reality of pieces of metal in our left foot whenever we complain that we have pins and needles in it. For, strictly speaking, the acceptance of entities in the internal sense—and it is only in this sense that Carnap seems to accept any entities at all—is in fact the acceptance of certain linguistic forms. If this is so then the reviewer sees no reason why it should not be conceded to Carnap that, contrary to the argument of some nominalists, the acceptance of linguistic forms involves us in no ontological commitments. After all, the ontology we embrace should not depend on the language we use but rather on what we assert with the aid of this language.

Let us, however, consider certain specific problems of semantics. In accordance with Carnap's intentions the semantical statement (1) 'The word "Chicago" designates a thing' is to be interpreted as indicating that the object language which is presupposed by our semantics, is a *thing language*, i.e. a language within the framework of which we have (a) constant names, (b) a constant predicate which with any name as argument forms a true statement, and (c) variables for which names can be substituted. Moreover, (1) is to be regarded as analytic as it follows from the statement (2) 'The word "Chicago" designates Chicago', which is supposed to be analytic in virtue of the general principle that any expression of the form "... " designates ... ' is an analytic statement, provided the term "... " is a constant in an accepted framework (p. 217). In this connection one might remark that this is far from being a self-evident interpretation of the expression 'designates a thing'. And as regards the general principle it seems to be

sufficient to put the word 'or' in the place of the dots in order to undermine its alleged validity.

As far as the reviewer can judge, the word 'designates' is quite commonly used in a somewhat different sense. One way of characterising this sense informally would be to postulate the truth of at least the following statements: (3) for all x , y , and z ,—if x designates y and x designates z then y is the same thing as z , (4) for all x , y , and z ,—if x designates z and y designates z then the corresponding statement of the form ' a is the same thing as b ' with x and y as the arguments is true, and (5) for all x and y ,—if x designates y then y is real. Let us make it clear at once that the concept of reality which is presupposed in (5) is, to use Carnap's terminology, an 'empirical, scientific, non-metaphysical' concept. Under such an interpretation statements (1) and (2) appear to be empirical statements. If it is empirically or factually true that the word 'Chicago' designates something then it seems to be legitimate to ask whether every expression of our language has the same property or not. In particular it seems to be legitimate to ask whether expressions like 'Pegasus, is large', 'Chicago is large', have any designata or not. The critics of the "Fido"—Fido' principle in semantics maintain that these expressions designate nothing, and that only names, except those which are fictitious, can designate. The term 'designates' in the sense just explained, however, seems to have no place in Carnap's semantics, which in the light of the present essay turns out to be a theory of linguistic forms rather than a theory of relations that may obtain between the expressions of a language on the one hand and the things which constitute the world on the other.

The reviewer is left with the impression that Carnap is more successful in meeting the criticism based on the Quine-Goodman nominalistic doctrines, and that his appeal for tolerance in permitting the use of a wider variety of linguistic forms is likely to command considerable sympathy. In dealing with the critics of the "Fido"—Fido' principle in semantics Carnap does not seem to be so successful. For the immunity which he secures against their objections, rests on what is likely to be regarded as an interpretative twist. If Carnap's semantical method has been misunderstood and misinterpreted by his critics owing to their failure to distinguish between the internal and the external questions of existence then it is not the critics who ought to be blamed for the confusion.

C. LEJEWSKI

Probability and Scientific Inference. By G. Spencer Brown.

Longmans, Green and Co., London, 1957. Pp. ix + 154. 15s.

In fairness to the author it should be stated that he and the reviewer indulged in an ardent controversy on the first day of the Third London Symposium

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on Information Theory,¹ and readers of this review may therefore consider it appropriate to tone down some of the adverse comments.

In spite of its faults the book is entertaining, succinct, and provocative, and is written with some imagination. In fact the author could probably write good imaginative prose. As a writer on technical matters he is too fond of propounding paradoxes and leaving them unresolved for too long.

The main theme is that the foundations of existing theories of probability are self-contradictory, at any rate if the applications are included. The argument depends mainly on criticisms of the idea of a random sequence, and may be regarded as directed largely against the von Mises philosophy of probability, which is based on the idea of an infinite random sequence. It has in fact been recognised for many years that infinite sequences cannot have any direct operational significance; nevertheless there is no mathematical contradiction in the idea² (although the author seems to imply that there is at the end of p. 56).

Modern theories of probability, which are based on such axioms as $P(A.B|C) = P(A|C) P(B|A.C)$, do not depend on the idea of infinite random sequences, but this idea is found to be mentally convenient in the applications. For example, if we have a machine that is supposed to produce 'perfectly' (equiprobably) random digits, then any finite sequence, S , produced by it, can be thought of as a finite segment of an infinite sequence, and such a segment may be called a 'finite random sequence'. Every finite sequence of digits, even say 3333333333, can occur in the output of a perfect randomiser. In practice if a sequence like 3333333333 is presented to us by itself, and not merely as a part of a sequence of about 10,000,000,000 digits, then we tend to believe that it did not come from a perfect randomiser by means of a place selection. (A place selection is one that is made in ignorance of the digits that will be obtained.) The reason for this belief is that the probability of obtaining this sequence is much higher given various alternative hypotheses.

It should be well known that the objectivity of orthodox statistics is obtained only at the expense of ignoring information. (See, for example, the reviewer's book,³ p. 102.) Often the information ignored includes an exact statement of a choice of random numbers.⁴ It is possible for the result of an experiment to be significant and yet most of the significance may belong to an unlucky choice of random numbers. It is partly for this reason that the practical statistician requires to use his judgment. For reasons of propaganda this necessity for judgment is ignored in a great

¹ *Information Theory*, ed. Cherry, London, 1956, see pp. 8 to 18

² See, for example, p. 14 of the reference in footnote 1

³ *Probability and the Weighing of Evidence*, London and New York, 1950

⁴ See, for example, *Journal of the Inst. of Actuaries*, 1956, 82, 255

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many textbooks on statistics and in science in general: the writers are too anxious to establish the glorious objectivity of science. By pretending that science is entirely objective they reveal their own lack of objectivity.

A long sequence of digits obtained from a good randomiser is bound to contain some very peculiar short sequences. A statistician who finds at the end of his experiment that he has used a peculiar subsequence may be worried about it. For this reason he may decide to avoid the use of peculiar subsequences. But then, as Spencer Brown says, the statistician would not be using strictly random numbers. Most statisticians would prevent this difficulty from becoming serious by being careful not to be too fussy, i.e. they would reject only very peculiar subsequences. Therefore it seems unlikely that the difficulty will cause many sleepless nights for statisticians.

But suppose that the statistician is very fussy. Then, says Spencer Brown in effect, he will have so many criteria of perfect randomness that he will reject nearly all subsequences. The author's argument here is that if each test lets through 95 per cent of the subsequences that are still left, then only a proportion $(0.95)^n$ will be left in after n tests, and if n is large the proportion will be negligible or zero.

The natural answer to this argument is that the criteria, for subsequences of a fixed length, should first be roughly ordered in ascending order of complication. The significance levels for reasonable rejection will then not all be 0.05, but will form a sequence $P_1, P_2, P_3, \dots$, where $P_1 > P_2 > P_3 > \dots$. It will then be possible to have a large set of tests and yet not to reject a high proportion of subsequences of the given length. The usual applications of random numbers in statistics will then be approximately valid.

Apart from this theoretical objection to the use of random digits the author believes that no method of producing perfectly random digits is satisfactory and that this throws doubt on statistical experiments, especially on those designed to test E.S.P. (extra-sensory perception). This objection is not supposed to be of the kind that no physical probability can be 0.5 correct to an infinite number of places of decimals, which would be a trivial objection; it seems to be rather that good reasons for rejecting the perfect randomness can always be found without a very great expenditure of effort. This view is published at a time when it should not be hard to disprove, since methods of producing random numbers are at present becoming very efficient with the rapid advance in electronics.

The author gives some examples of significant deviations from randomness in the tables of Fisher and Yates, of Tippett, and of Kendall and Babington Smith. The highest significance level reached is $1/7340$, which is hardly comparable with the best results in E.S.P. experiments. For example, Soal and Bateman¹ summarise a sequence of experiments that reach a

¹ *Modern Experiments in Telepathy*, Faber and Faber, London, 1954, pp. 425, p. 311

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significance level of about 10^{-70} . The author's reply to this would be (p. 87) 'p-values [tail-area probabilities] like 10^{-35} , which are so much prized in a certain type of investigation, cannot possibly give us greater certainty than, at the outside, a p-value of 10^{-3} ; and a p-value of no less than 10^{-2} serves in nearly all cases as the most stringent practicable criterion, since if an experiment is at all difficult we are not likely to do it more than 100 times.' It seems to the reviewer that a tail-area probability of 10^{-35} is in an entirely different class from one of 10^{-3} , irrespective of whether the experiment is to be repeated. It would be easy to reach a significance level of 10^{-3} by making say 100,000 independent tests for significance. (This would be practicable on a modern computer, or manually, if enough people became interested.) But to obtain a level of 10^{-35} for a 'finite random sequence', which had already passed several of the simpler tests, would be very difficult, except by means of tests constructed artificially after looking at the sequence.

If the reputation of the theory of probability is at stake it is to be hoped that one of the American research corporations will decide to produce a fully reliable randomiser, and will publish some of its output without any editing. If 1,000 readers of Spencer Brown's book perform tests on this output, then of course some of them may get tail-area probabilities in the region of 10^{-3} . Unfortunately there is then a danger that only the significant results will be announced! Therefore it would be better if a set of tests could be performed by the publishing corporation and the results of all of them published with the random numbers. When this is done it would be important that the serial test, first described by Kendall and Babington Smith, should be given its correct interpretation, as found by the reviewer.¹

If the author turns out to be right that no proper randomiser can be produced then the reviewer would regard the result not as a contribution to the theory of probability but rather to theoretical physics, or to psychokinetics.

A difficulty concerning orthodox statistics is mentioned on page 65. Fisher is quoted as saying that (a) no isolated experiment, however significant in itself, can suffice for the experimental demonstration of any natural phenomenon, (b) any series of experiments may be regarded, for the purpose of statistical argument, as a single or isolated experiment. The author draws the obvious conclusion and describes it as a 'beautiful paradox'. On looking up the original reference² we find that Fisher went on to say that 'what is really wanted is a reliable method of procedure' and not just an isolated record. Nobody would quarrel with this as a *desideratum* but surely a tail-area probability of 10^{-35} cannot be ascribed to chance. The

¹ 'The serial test for sampling numbers and other tests for randomness', *Proc. Cam. Phil. Soc.*, 1953, 49, 276-284

² *The Design of Experiments*, 1949, pp. 13-14

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reason why many statisticians are unconvinced by such tail-area probabilities in E.S.P. experiments is that they fear that the experiments may not have been properly performed or reported, and the reason why they pick on E.S.P. for their suspicions is that they regard the initial probability as small that there is anything in it. The general belief in E.S.P. would be increased more if new experimenters were to obtain significant results than if the same old experimenters were to continue to slash their previous tail-area probabilities.

Apart from the criticisms of probability and statistics, the book contains a few chapters on other philosophical questions. For example, Chapter 4 makes a point made by von Wright previously,¹ a point that may be put in a nutshell as follows :

A meson is lighter than a proton.

What is the reason ?

For if it were heavier, none dare call it meson.

On page 83 there is a reference to 'Wisdom's Cow'; a cow generally has a number of properties no one of which is essential to its cowness. It is surprising that the paradox connecting the meson and the cow is not mentioned, in view of the author's love of paradoxes. The paradox can be resolved by the following definition of definitions of things : Let $Q_1, Q_2, Q_3, \dots$ be a sequence of qualities. Let $x_i = 1$ or 0 depending on whether anything, T , has quality Q_i . Then T is a meson (or cow) if $f_j(x_1, x_2, x_3, \dots) > 0$ ($j = 1, 2, \dots$), where the functions f_j define a meson (or cow). Often a single linear or quadratic form would be adequate. This definition of definitions of things is analogous to the use of discriminant functions in statistics.

On page 19 it is argued that the 'Law of Uniformity cannot be falsified ; for when our observations are not uniform we say we are not observing nature'. The reviewer thinks that we should usually prefer to say that the 'circumstances' were different or that the relevant laws of nature were only probabilistic.

I. J. GOOD

Thomas of Bradwardine His Tractatus de Proportionibus.

Edited and translated by H. Lamar Crosby, Jr.

The University of Wisconsin Press, Madison, 1955. Pp. xi + 203. \$3.50

THE rehabilitation of medieval mechanics which was begun early this century by Pierre Duhem in Bordeaux and has recently been furthered with great distinction by Anneliese Meier in Rome, is now being continued in the United States, notably by the publications of the University of Wisconsin

¹ *A Treatise on Induction and Probability*, Routledge and Kegan Paul, 1951, pp. 141-146

Press. Under the editorship of Marshall Clagett the texts and translations of key works are becoming generally available.

Outstanding among the fourteenth-century contributions to medieval kinematics and dynamics was the treatise on the 'proportions of the speeds of motion' written in 1328 by Thomas of Bradwardine before he left Merton College, Oxford, to become successively Chancellor of St Paul's, Chaplain-Confessor to Edward III (in whose service he saw much action in the field, e.g. Crécy), Archdeacon of Norwich and finally in 1349 Archbishop of Canterbury. This office he held for barely a month before he succumbed, like so many other great men of the time, to the Black Death.

Even in his own day, despite his ecclesiastical eminence, Bradwardine was esteemed primarily for his intellectual achievements and was dubbed 'Doctor Profundus'. His short tract on 'proportions'—or, as we should now say, 'functions'—is of outstanding importance in the history of mathematical physics because it appears to have been the first attempt to analyse physical phenomena in terms of more sophisticated mathematical relations than simple linear proportions. Consequently, although the particular relations considered by Bradwardine are now regarded as inappropriate for the purpose intended and moreover his entire discussion is purely *a priori* without any appeal to experiment, we must salute him as one of the great pioneers of modern scientific method. Despite its ultimate complete supersession in the seventeenth century, his tract was of great influence on subsequent generations, as is evident both from the comparatively large number of manuscript copies which still survive and from the fact that it was printed in many editions in the late fifteenth and early sixteenth centuries in such places as Paris, Venice, Vienna, Valencia, and Wittenberg. In view of the complete absence of Bradwardine's theological writings from the early printed editions of his works, Dr Crosby concludes that it would seem that his work in natural philosophy was of considerably greater interest than his theological writings to men at the beginning of the sixteenth century.

Dr Crosby's scholarly book must be welcome first and foremost because it makes easily available for students both the original Latin and an excellent English translation of the *De Proportionibus*. The text is based on a collation of four fourteenth-century manuscript versions, three other less reliable manuscripts being used for auxiliary checking. The text and translation are printed on alternate pages facing each other. Variant readings and a most useful series of notes are printed at the end. The Introduction, which runs to nearly sixty pages, comprises a short biography of Bradwardine, an annotated list of editions and manuscript sources, a short essay on the general significance of the tract and a lengthy detailed critical analysis.

The central problem of Bradwardine's tract concerns the relation of dynamics to kinematics. His theory of the relation of motive force F ,

resistance R and velocity V can be expressed, in modern notation, by the formula $V = \log_n (F/R)$. This peculiar relation, which he advanced in preference to the Aristotelian simple F/R proportionality, was the result of his struggle to produce a satisfactory formula in the absence of any clear recognition of the distinction between inertial and other resistances to motion, e.g. frictional, a problem which was only finally resolved with Newton's explicit formulation of the concept of mass. The origin of Bradwardine's formula may be briefly summarised as follows. It was realised by Aristotle himself that his law, V proportional to F/R , cannot apply when, as often happens, F is insufficient to cause motion. Several medieval writers made suggestions for overcoming this difficulty. Bradwardine restricted his attention to cases where motion does occur and also interpreted Aristotle's law of proportionality in a peculiar sense suggested to him by the rule given in a commentary on Euclid that, if $a/b = b/c$, then $a/c = (a/b)^2$. He argued that, if V is produced by F/R , then $2V$ will be produced not by $2F/R$ but by $(F/R)^2$.

Particularly noteworthy in Bradwardine's analysis is his great advance on Aristotle in regarding velocity as an instantaneous 'quality' of motion and not merely as a simple quotient obtained by dividing distance by time.

The great defect of Bradwardine's work, when judged irrespective of his neglect of empirical considerations but on its own terms, is the extreme poverty of his mathematical notation. Indeed, it would seem that this deficiency, which was characteristic of the fourteenth and fifteenth centuries no less than the absence of a proper appreciation of the interrelation of theory and experiment, delayed the rise of classical dynamics for several centuries. (The publication in 1638 of Galileo's *Two New Sciences* comes almost midway in time between Bradwardine's tract and the present day.) The development of key kinematic ideas, such as acceleration, became scholastic achievements of extraordinary difficulty and subtlety. In the development of such ideas the Merton school of the fourteenth century, of which Bradwardine was an outstanding member, played a vital rôle grossly underestimated by Duhem in his enthusiasm for the great fourteenth-century French school of Buridan, Oresme, etc. Nevertheless, Crosby seems to overshoot the mark when he claims that Bradwardine's pupil John of Dumbleton interpreted his master's formulation of dynamics in a manner akin to Newton's second law of motion.

Although, in the present very incomplete state of our knowledge of fourteenth-century dynamics, only a handful of experts can even begin to adjudicate on the questions at issue, the crucial significance of that century in the evolution of scientific method makes it highly desirable that books such as Dr Crosby's should be as widely read as possible by philosophers of science no less than by historians.

G. J. WHITROW

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Existential Psychoanalysis. By Jean-Paul Sartre.

• Philosophical Library, New York, 1953. Pp. viii + 275. \$4.75

THIS is a translation of some final chapters of *L'Être et le néant*. It is a contribution both to psychology and to the philosophy of psychology; in addition it contains metaphysic. It seems to me important.

It is better, according to Sartre, to define a man by his aims rather than by his desires; then what we want to understand is not the choice of an aim in general, but why this particular man has this particular aim, and this involves finding his 'original project'. Every other project, such as going rowing, is a manifestation of the original project in particular circumstances.

The original project is the aim simply of being—'man is the desire to be' (p. 63). At this point arises a distinction between 'being-in-itself' and 'being-for-itself' (terms that will hardly endear themselves to the English-speaking world). 'Being-for-itself' is the reality a man lacks, what he has not become, and to the extent that he lacks it he lacks being. His project is to overcome this lack. 'Existential psychoanalysis'

is a method destined to bring to light, in a strictly objective form, the subjective choice by which each living person makes himself a person; that is, makes known to himself what he is. Since what the method seeks is a *choice of being* at the same time as a *being*, it must reduce particular behavior patterns to fundamental relations—not of sexuality or of will to power, but of *being*—which are expressed in this behaviour (p. 87).

Sartre turns to show that there are three fundamental categories, *to make*, *to have*, and *to be*; on examination, however, he finds that the first two reduce to the third (later on he mentions that there is never being without having, but being in some way remains more fundamental).

En passant there are several striking comments, where he discusses the 'digested indigestible', finds that 'desire is the desire of devouring', shows that the serious man is in 'bad faith'. He proceeds to locate the fundamental form of desire in appropriation—pointing out that the reason why some peoples buried their dead with their belongings was not because of a belief that the dead man could use these things but simply because they belonged to him, in the sense of forming part of him. Thus he analyses possession in terms of being united with an object considered as appropriated; the desire for a certain thing is not just the desire for it, but the desire to be united with it (which shows that 'appropriation' is not just 'owning' but incorporated into one's being). In this way he shows that *to have* is reducible to a relation of *being*. Again, he claims that a gift is a 'primitive form of destruction' (p. 141).

If then existential psychoanalysis encounters evidence of *generosity* in a subject, it must search further for his original project and ask why the subject has chosen to appropriate by destruction rather than by creation. The answer to this

question will discover that original relation to being which constitutes the *person* who is being studied (p. 143).

In general,

The goal of existential psychoanalysis is to rediscover through . . . empirical, concrete projects the original mode in which each man has chosen his being (p. 155).

Sartre next proceeds to a 'psychoanalysis of things', which the well known philosopher of science, Bachelard, tried to do, we are told, for *water*. This, one may intervene to say, is not so fantastic as it may seem. Sartre proceeds to his famous analysis of the *slimy* (pp. 168-89), which consists of a phenomenological—and in his view objective—description of all kinds of facets of the experience of the slimy. It is with this form of analysis that Sartre is at his best. The point of this phenomenological analysis is that it reveals the *being* of the slimy. So all things, human and inanimate, have their being in modes of being.

Finally Sartre enters upon the question of 'bad faith'. 'Bad faith' is ambivalence towards oneself. It is not, of course, to be identified with lying, for in bad faith one is not aware of the false note—and yet one is not wholly unaware either, one somehow knows that one is taking oneself in. And it is not this duplicity towards another but towards oneself that constitutes bad faith. Sartre gives fine descriptions of instances, such as that of a woman who keeps from herself the admission that she is going to be seduced but whose actions made no sense unless they lead up to this. A different sort of example is that of a waiter, who must if he is to satisfy his clientèle play the part of being a waiter, which is not the same thing as carrying out the acts that constitute waiting—he is therefore what he is not, in the Hegelian language of Sartre. Sartre concludes that there is an inherent conflict in bad faith: it seeks 'to flee what it cannot flee, what it is' (p. 268); and there is even bad faith in the original project of bad faith, for it could not be adopted with sincerity (p. 261).

Though not entirely plain, I think the overall point here is that bad faith or the ambivalence that constitutes it either prevents a man from achieving being or is a manifestation of failure to be.

I do not know what readers will make of this. I would suggest it would be worth their while getting the text and struggling with it. M. Sartre has come to be known in this country for two unfortunate things: the author of obscure or misleading saws, such as 'man fundamentally is the desire to be God' (p. 67) or 'man is a useless passion' (p. 199), which are very fine when understood as aphorisms displaying what they epitomise; and the purveyor of bad—even appalling—logic.

It is quite true that Sartre has expounded some of his thought in a form that passed for logic, and very bad logic it is, though not so gross as it is sometimes represented. But there is no necessity whatever to expound

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his views in this way (though no doubt if we are to apply the ideas of the present book we shall be reaching the being of Jean-Paul if we study him in his chosen medium of quasi-logic, and otherwise we shall be getting existentialism without a lacing of Jean-Paul); and I wish to suggest that the content, once the logical trappings have been lost in oblivion, is important. I do not know whether the pseudo-logical guise appeals to the illogical nature of the logic-loving French, but it has probably done the author undeserved harm in England where the accurate use of logic is something of a fetish. M. Sartre is surely not only an outstanding French philosopher, and one would perhaps have to go back some centuries to find a French philosopher to equal him, but he has certain qualities that put him almost alone in the world of contemporary philosophy: he is thoughtful, has something to say, he is interesting, and what he has to say gives the impression that it matters.

Sartre's theme is man—or rather this particular man *vis-à-vis* other men. He has a theory about their interrelations which is a theory of being and the free act. His theory of existentialism might be put like this:—Confronted with a situation in which a decision must be taken and an action carried out, yet lacking sufficient causal factors derived from convention, or moral or religious codes, to determine the decision or the action (for to be determined by these would be to have chosen bad faith as a basic principle of life), then the person involved, without motive for decision or action, experiences a chasm between him and the decision and action that must be taken, which gives rise to the experience of anxiety; but at that very moment he is free to decide and act, and his free move is a leap across the chasm, abolishing it, taken in accordance with his fundamental choice of project as the basic form in which he desires to be. (The free leap is, it is true, determined by the project, but though it could be interpreted as a motive the project is not a motive in the ordinary sense.) Though not devoid of methodological difficulties, this seems to me to make very good sense—at least on the phenomenological level, as most of it is.

It is perhaps a pity that Sartre calls his work 'existential'. No doubt the reasons are historical and connected with his queer logic about existence, and there is the good point that his philosophy is concerned to emphasise the immanent. Still 'existential' is not a very graphic word for this. It is also a pity he used the word 'psychoanalysis', for his practical procedures seem to bear almost no relation to psycho-analysis. The reason no doubt is a certain kinship between his theory and the theory of psycho-analysis. The nearest I can get to an adequate phrase is Psychogenetic Phenomenology.

It is not surprising therefore that he discusses Freud in some detail. He outlines his very considerable agreement with and his very considerable divergence from psycho-analysis. His interesting criticisms of it appear novel and may well prove important. This would take some sifting,

REVIEWS

because his excellent presentation of Freud's work contains one or two slight—very slight—errors ; but to investigate this would require a paper to itself. This theme is very much philosophy of science.

I should like to end by emphasising that this, though a book on philosophy of science and on philosophy, is largely a book on psychology. This alone gives it a place for our attention, since so few books on psychology have ever been written.

J. O. WISDOM

ABSTRACTS

Dialectica, 1956, 10, No. 1

F. Gonseth, 'Dialectica entre dans sa dixième année d'existence'

The original intention was to produce an organ for 'open philosophy'. This start has been maintained in spirit though not adhered to with complete rigour.

N. Rotenstreich, 'Kant's Schematism in its Context'

The article attempts to analyse the position of schematism in Kant's theory of empirical knowledge. The view suggested in the article is that it is the idea of schematism and not the idea of transcendental deduction which is Kant's real reply to Hume's question. Empirical knowledge according to Kant is possible because time itself, being the mediating link between concepts and data, contains a minimum of order and hence a minimum of rationality.

H. Binder, 'Der Begriff des Normalen in der Psychiatrie'

The criterion of the mentally normal is that the psychic disposition, which always tends to maintain or restore the balance of the various psychic functions, is unimpaired. Since this striving for balance can assert itself throughout the psyche despite the existence of partial disharmonies and conflicts, the latter still form part of normal life. In all non-normal subjects, the inner tendency towards psychic balance is impaired and this causes fixed disturbances of the inner equilibrium; this reveals itself in a consistently displayed inadequacy of behaviour.

E. Simenauer, 'Betrachtungen über die biologischen Grundlagen des Denkens und des Willens'

The philosophy of perception and knowledge has lately been confronted with the establishment of the psychological basis of ideation. Contemporary attempts at correlating the concepts of depth-psychology to neuro-physiological theories are broadening the biological approach to enquiries which have been traditionally reserved to philosophy. In this preliminary and programmatic communication reference is being made to one of these neuro-physiological theories (W. Scheidt) which endeavours to describe conscious and unconscious thinking in terms of biological processes taking place in the various neuro-fibrillary systems. Thus a biological theory of knowledge is emerging which holds out hope of fruitful co-operation between philosophy and science.

M. Gex, 'Chronique de caractérologie. La caractérologie de René La Senne'

René La Senne has made a school of characterology of the Dutch writers Heymans and Wiersma, known throughout France. This purely psychological characterology composes types of character, utilising three basic factors, emotivity, activity, and

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reverberation which allows one to assign to each type a description analogous to that of a chemically composed substance. From varied studies, ranging from literary history to psycho-sociology, show the fertility and flexibility of this method of analysis of character.

H. Dingler, 'Geometrie und Wirklichkeit (Forts.)'

This continues the article in the previous Number.

Dialectica, 1956, 10; No. 2

F. Gonseth, 'De l'humanisation de la technique'

The world of technicians sometimes takes note of its powers and responsibilities. A great problem opens up which may be analysed more profoundly by introducing the concept of feed-back from cybernetics. The author also discusses certain other conditions inseparable from the intention to humanise technique.

Y. Reenpaa, 'Versuch über die Beobachtungsgrundlagen der exakten empirischen Wissenschaften'

This is an attempt to explain certain difficulties in the philosophical understanding of the natural sciences by examining in one definite case (the so-called relation of uncertainty) the structural problem of an aprioristic observation and by advancing from this starting-point to the conceptual side of the problem.

The special case which the writer tries to examine thoroughly is that of visual minimum perception. The conclusion is as follows: the so-called full minimum perception (minimale Vollbeobachtung), i.e. a perception which is minimal in its duration, area, and intensity and which is, in its colour quality, determined, can be depicted in the g -cm-sec-system as a quantity of the dimension of the Wirkungsgrösse (dimension $g \text{ cm}^2 \text{ sec}^{-1}$). By calculating the 'Wirkung' in question we get a value of 1 to 8 quanta. This result is compared with those given in papers by Hecht, Schlaer and Pirenne and by Bouman and Baumgardt concerning the minimum number of light quanta necessary for visual excitation.

Finally it should be mentioned that the conceptual representation of the minimum perception includes the question of uncertainty, because this perception is a simultaneous or coincidental perception.

A. Pap, 'Logical Empiricism and Rationalism'

According to the version of logical empiricism which Louis Rougier advocates (*Traité de la connaissance*) in opposition to 'a priori rationalism', necessary truths are merely the consequences of freely adopted linguistic conventions. Rougier attempts to substantiate this 'conventionalism' by reference to pluri-valued logics in which the laws of two-valued logic allegedly do not hold. But he confuses *sentences* and *propositions*: what depends on conventions is only that a given sentence expresses a necessary proposition, not that the proposition expressed is necessary. It still does not make sense to say that logical truths are arbitrary postulates or consequences of such, for what is called a logical truth is not a formal postulate but the *proposition*

ABSTRACTS.

that whatever interpretation satisfies such and such formal postulates also satisfies such and such formal theorems.

• Rougier's theory of empirical knowledge corresponds, in rough outline, to the phenomenalism and unduly stringent (even self-contradictory) verifiability principle of meaning which dominated an early phase of logical empiricism, and is therefore outdated. Moreover, his exposition of early doctrines of the Vienna Circle is infected with logical blunders, such as the confusion of propositional functions and universal propositions. His critique of metaphysical pseudo-statements and pseudo-problems that arise from the imperfections of the syntax of ordinary language, is likewise outdated as compared with recent linguistic analysis in England.

F. de la Horbe. 'Sociologie et philosophie : Hommage de la sociologie à l'idéonisme'

The history of theories displays oscillation between the two poles of belief and experience. The development of the exact sciences is based on the predominant influence of the latter while moral and social theories remain more or less prisoners of philosophy. It was only in the nineteenth century that sociological theories tended to become scientific. This article explores the philosophical basis of sociology. This composite science offers a spectacle of scientific analysis being replaced by verbal classifications. In this difficult phase ideonism opens the way towards a doctrine favourable to elementary truths.

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No. 36

THOUGHTS ON TELEOLOGY *

ISRAEL SCHEFFLER

Introduction: Two Strategies in Interpreting Teleology

How shall we relate teleological notions, referring events to certain of their consequences, with causal notions, referring events to certain of their antecedents? Two hard-headed strategies are discernible in the recent literature—hard-headed because in neither one are teleological statements taken to refer to special sorts of entities such as entelechies nor to embody special sorts of explanation by means of final causes. One such strategy is to interpret teleological statements as descriptions of plastic or self-regulating behaviour, in principle explainable in ordinary causal terms. The other strategy is to construe ostensible mention of goals future to an action as referring rather to ideas of such goals prior to the action, and hence capable of figuring in its normal causal explanation.

Each of these strategies has occasionally been used in such a way as to provoke the charge of misapplication. The first has thus, for the most part, been criticised for failing to do justice to purposive action in the higher animals and man, the second for unplausibly ascribing goal-ideas to the lower animals and to non-purposive human behaviour. These criticisms, however, leave unscathed a moderate use of either strategy within well-defined limits: so long as you do not try to reduce *purpose* to self-regulative behaviour, nor attribute *ideas*, *wishes*, or *beliefs* to behaviour that is not consciously purposive, you may proceed without hindrance.

It is this moderating assumption that I shall examine in the two main sections of the present paper: (i) First, with respect to two

* Received 11. vi. 58. For certain critical suggestions, I am indebted to Professors N. Chomsky, N. Goodman, and M. Mandelbaum, who should not, however, be assumed to share the views presented here.

recent interpretations of teleology in terms of self-regulative behaviour, I shall argue that they are inadequate not merely for purposive, but also for certain sorts of non-purposive behaviour, for which I will suggest the possibility of an alternative treatment. (ii) Secondly, I wish to indicate some serious logical-semantic difficulties attending purported references to beliefs about and desires for a goal even in accounting for fully purposive human action, and I want to explore a way of handling such difficulties.

1 *The Self-regulation Strategy*

(A) *Negative Feedback.* Perhaps the most basic recent paper applying the notion of self-regulation to the interpretation of teleology is that of Rosenblueth, Wiener, and Bigelow.¹ Convinced that 'purposefulness (is) a concept necessary for the understanding of certain modes of behaviour',² and that its importance has been slighted as a result of the rejection of final causes, these authors propose to explain purpose, in largely engineering terms, as behaviour 'that may be interpreted as directed to the attainment of a goal, i.e. to a final condition in which the behaving object reaches a definite correlation in time or in space) with respect to another object or event'.³ In order to accomplish this explanation, the authors introduce the terms 'input', 'output', and 'feedback', as follows. 'Input' applies to events external to an object that modify the object in any manner. 'Output', on the other hand, refers to changes produced in the surroundings by the object. 'Feed-back' may be used in two different ways. It may be applied to objects (such as electrical amplifiers), some of whose output energy is returned as input. In such cases feed-back is positive in that the output re-entering the object 'has the same sign as the original input signal',⁴ thus adding to this signal rather than correcting it. On the other hand, feed-back is negative when the object's behaviour is 'controlled by the margin of error at which the object stands at a given time with reference to a relatively specific goal'.⁵ In such cases, 'the signals from the goal are used to restrict outputs which would otherwise go beyond the goal'.⁶

With the notion of negative feed-back at hand, the authors propose that teleological behaviour be construed as behaviour controlled by

¹ A. Rosenblueth, N. Wiener, and J. Bigelow, 'Behaviour, Purpose and Teleology', *Philosophy of Science*, 1943, 10, 18ff.

² *Ibid.*, p. 23

³ *Ibid.*, p. 18

⁴ *Ibid.*, p. 19

⁵ *Ibid.*

⁶ *Ibid.*

negative feed-back.¹ 'All purposive behavior', they say, 'may be considered to require negative feed-back. If a goal is to be attained, some signals from the goal are necessary at some time to direct the behavior'.² We may, of course, construct a machine that will impinge on a luminous object although the machine is insensitive to light, as well as to other stimuli emanating from the object. It would however, be a mistake to consider such impingement behaviour purposive inasmuch as 'there are no signals from the goal which modify the activity of the object *in the course of the behavior*'.³

By contrast, some machines are 'intrinsically purposeful', for example, 'a torpedo with a target-seeking mechanism'.⁴ The behaviour of such objects involves 'a continuous feed-back from the goal that modifies and guides the behaving object'.⁵ The path followed by the torpedo, for example, is controlled by the signals it receives from the moving target.

The foregoing conception of teleology is one whose scientific and practical importance is generally acknowledged and is surely not in question here. That it nevertheless fails adequately to characterise clear instances of purposive behaviour is an independent point that emerges from the following considerations.

The authors interpret purposive behaviour as directed toward 'a final condition' in which the object achieves correlation with respect to some other entity. Since they describe the goal of such behaviour as something that emits signals guiding the behaving object, the goal cannot be identified with the final condition of correlation mentioned above. For one thing, this final condition, if it occurs at all, is later than the behaviour in question and cannot therefore be supposed to guide it if the notion of final causes acting upon earlier events is rejected. For another, the authors describe cases of 'undamped feed-back', in which a purposive machine, by increasingly larger oscillations, overshoots the mark and fails to attain the final condition toward which its behaviour is directed, though such behaviour has presumably been modified by signals from the goal. The goal, as described by these authors, cannot, therefore, be identified with the final condition of correlation and must rather be construed as that with which correlation is supposed to occur, i.e. what we may call 'the goal-object'. This goal-object, even where correlation fails to occur, emits signals that modify the purposive activity of the behaving object.

¹ Ibid., p. 24

² Ibid., p. 19

³ Ibid., pp. 19-20, italics in original text.

⁴ Ibid., p. 19

⁵ Ibid., p. 20

The difficulty of the missing goal-object

• Now it is just the existence of such a goal-object that cannot be assumed in all cases of purposive behaviour. As pointed out in a critical paper by R. Taylor,¹ a man's purpose in groping about in the dark may be to find matches that are not there, his purpose in going to the refrigerator may be to obtain a non-existent apple, he may seek the philosopher's stone, the holy grail, the fountain of youth, or a live pulsing unicorn. In every such case his behaviour is clearly purposive and yet in none is this behaviour guided by signals emitted from a goal-object, correlation with which represents the final condition toward which the behaviour is directed. Generally, if a man's purpose is to obtain object *O*, we cannot infer the existence of something *x*, identical with *O*, such that he seeks *x*, nor can we infer, from the fact that a man's purpose is to obtain something of kind *K*, that there exists some object of kind *K* for which he strives.²

This criticism, it will be noted, establishes the inadequacy of the Rosenblueth-Wiener-Bigelow interpretation only with respect to purposive cases. Indeed, Taylor supposes that we have here an 'irreducible difference' between human beings and machines,³ suggesting thereby that what may be called 'the difficulty of the missing goal-object' does not arise for non-purposive cases on the Rosenblueth-Wiener-Bigelow account. As against this suggestion, I now argue that the same difficulty arises also with respect to non-purposive teleological behaviour in non-human as well as human organisms, and conceivably in machines too.

Consider a standing passenger thrusting his foot outwards suddenly in order to keep his balance in a moving train, a rat depressing the lever in his experimental box in order to secure a food pellet, a small infant crying in order to attract mother's attention. To describe each of these cases as has just been done is to provide a teleological account relating the behaviour in question to some selected end. It is not, however, to attribute purpose to the organism concerned, in the sense of our previous instances, for example. If the knight's purpose in

¹ R. Taylor, 'Purposeful and Non-Purposeful Behavior: A Rejoinder', *Philosophy of Science*, 1950, 17, 329

² Such inferences, invalid in cases where we have an apparent reference to a goal-object, cannot even be formulated naturally for purposive cases lacking such reference. The mystic striving to become more humble through spiritual training is not moving toward correlation with some object, guided by signals emanating from it.

³ Op. cit., p. 330

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travelling far and wide is to find the holy grail, he has chosen to search for it in the hope of finding it. If a man's purpose in opening the refrigerator door is to get an apple within, he opens it with the intention of getting an apple within. It would in most cases be peculiar, however, to describe the train-rider's sudden kick as a product of choice or as an expression of intention. Psychologists, similarly, often feel uncomfortable in describing the rat as choosing to depress the lever, or as depressing it with the intention of securing a pellet. Finally, we should not generally say of a small infant, even when it cries in order to attract mother, that it has chosen to cry with the intention of attracting mother—though it may of course do the latter at a later stage, when its crying will be interpreted quite differently by its elders. I suggest these cases merely as examples of a large group for which we willingly offer teleological accounts but withhold ascriptions of intention, choice, and purpose.

It does not take much reflection to see that the difficulty of the missing goal-object arises even for the moderate attempt to apply the Rosenblueth-Wiener-Bigelow interpretation to this set of non-purposive cases. For we cannot plausibly suppose our train-rider to be receiving guiding signals from some region with which his foot is to be correlated. Neither, when the psychologist stops replacing the rat's pellets, can we describe the rat as receiving directive signals from some such pellet. Nor, finally, when mother, expecting baby to sleep, steps out to the corner store, is she available for the issuing of signals guiding the infant's behaviour toward final correlation with herself. The Rosenblueth-Wiener-Bigelow analysis cannot therefore be judged generally adequate even for non-purposive teleological behaviour, nor does it seem to suffice even when we restrict ourselves to non-human cases.

Teleology and learning

A suggestion: For at least *some* non-purposive cases, I suggest that an interpretation in terms of learning may throw some light on teleological description, where analyses in terms of self-regulation fail. In the case of the infant, the suggestion is that our 'in order to'-description of its present crying reflects our belief that this crying has been learned as a result of the consequences of like behaviour in the past—more particularly, as a result of having received mother's attention. Having initially cried as a result of internal conditions *C*, and having thereby succeeded in attaining motherly solace, representing a type of

rewarding effect *E*, the infant now cries in the absence of *C*, and as a result of several past learning sequences of *C* followed by *E*. The infant's crying has thus been divorced from its original conditions through the operation of certain of its past effects.¹ These past effects, though following their respective crying intervals, nonetheless precede the present crying interval which they help to explain. The apparent future-reference of a teleological description of this present interval is thus not to be confused with prediction, nor even with mention of particular objects in the current environment, toward which the behaviour is directed. Rather, the teleological statement tells us something of the genesis of the present crying, and in particular, of the prominent rôle played by certain past consequences in this genesis. Such an account is perfectly compatible with normal causal explanation, and it indicates why goal-objects may very well be missing in some cases for which teleological description is appropriate. It shows also that we have in this respect no irreducible difference between human beings and machines, since machines capable of learning through the effects of their own operations are equally subject to such teleological description.

(B) *Plasticity of Behaviour.* The interpretation of teleology proposed by R. B. Braithwaite,² though it resembles the Rosenblueth-Wiener-Bigelow approach, is considerably different in detail and restricts itself explicitly to what Braithwaite calls 'goal-directed behaviour' as distinct from 'goal-intended behaviour',³ thus avoiding criticisms such as that of Taylor, based on purposive cases. Braithwaite's use of the self-regulation strategy is deliberately moderate; he wants to interpret 'all teleological explanations which are not reducible to explanations in terms of a conscious intention to attain the goal'.⁴

That the notion of *causal chains* is fundamental in the analysis of teleology is, for Braithwaite, shown by the fact that the organism's behaviour does not directly produce the goal; it constitutes, rather, part of a causal sequence of events progressing toward the goal. What, however, distinguishes teleological causal chains from all others with which science is concerned? Here Braithwaite follows E. S. Russell in proposing as the main clue 'the active persistence of directive activity towards its goal, the use of alternative means towards the same

¹ R. S. Peters, 'Symposium: Motives and Causes', *Arist. Soc. Suppl.*, 1952, 26

² R. B. Braithwaite, *Scientific Explanation*, 1953, pp. 319ff

³ *Ibid.*, p. 325

⁴ *Ibid.*

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end, the achievement of results in the face of difficulties'.¹ Such plasticity of behaviour is 'not in general a property of one teleological causal chain alone: it is a property of the organism with respect to a certain goal, namely that the organism can attain the same goal under different circumstances by alternative forms of activity making use frequently of different causal chains'.² To specify this plasticity further, Braithwaite provides a schematic description.

We have, let us suppose, some object b which, in the normal case, is to be taken as a physical or organic system. Every event at any time in b is assumed determined by the whole preceding state of b taken together with the set of actual field or environmental conditions causally relevant to this event. Consider now a causal chain c , comprised of events in the system b during a particular interval and following immediately upon the initial state e of the system. Suppose also that the set of actual field conditions causally relevant to the events comprising the chain is f . Then c is uniquely determined by the initial state e taken together with the set of field conditions f .

Causal chains may now be related to a goal gamma as follows. Every chain ending in a gamma-event without containing any other such event is a gamma-goal-attaining chain. Relative to a given initial state e of b , we may consider the class of all (possible) sets of field conditions f uniquely determining chains that are gamma-goal-attaining. This class is the variancy Φ , with respect to b , e , and gamma, comprising the 'range of circumstances under which the system attains the goal'.³

Plasticity may be attributed to the system when the variancy has more than one member, so that the goal may be attained under alternative environmental circumstances, though not necessarily by means of alternative causal chains. Teleological explanations assert that the behaviour in question is plastic, and are intellectually valuable to the extent that this plasticity is not asserted on the basis of known causal laws of the system's mechanism, but rather on the basis of past observation of the conditions under which similar behaviour has taken place. Further, teleological explanations predict the occurrence of some gamma-goal-attaining chain on the basis of knowledge of the system's

¹ Quoted from E. S. Russell, *The Directiveness of Organic Activities*, p. 144, by Braithwaite, op. cit., p. 329

² Braithwaite, op. cit., p. 329

³ Ibid., p. 330: I do not discuss at length the problem of determining the beginnings of goal-attaining chains, on this view. Strictly, according to the definition given, a cat's capture of its first mouse ends a single mouse-attaining chain that began with the birth of the cat.

plasticity; they predict, in other words, that the set of field conditions that will in fact occur is a member of the appropriate variancy Φ , on the ground that Φ includes the class Ψ of all sets of field conditions likely to occur.

It is at once apparent that, since Braithwaite does not construe goal-direction in terms of a relation to some goal-object but rather in terms of the variability of appropriate field circumstances, the difficulty of the missing goal-object does not arise in his scheme. Fido's pawing at the door need not be controlled by signals from outside; it need only lead to his being let out under alternative circumstances, say when his master is at home or when only his master's young son is at home. The rat need not, as he depresses the lever, be receiving any stimuli from the pellet that drops into his cup only after the lever is depressed; it is enough that his depressing of the lever results in his obtaining the pellet under a variety of experimental conditions.

The Difficulty of Goal-failure

Braithwaite's interpretation is, however, subject to other difficulties. For he appears to hold that particular teleological explanations do not merely ascribe plasticity to the behaviour being explained but also predict goal-attainment in the circumstances that will in fact ensue. It is perhaps the least of the difficulties of this view that its use of the term 'explanation' is strange, bearing little relation to normal causal explanation. Of crucial importance, however, is the fact that teleological descriptions, whether or not they qualify for the title 'explanation', do not generally predict the attainment of the goal indicated. It is interesting that, despite the requirement of Rosenblueth, Wiener, and Bigelow for a *goal-object*, they allow for non-attainment or *goal-failure* in their examples of undamped feed-back resulting in overshooting the target. But it is easy to multiply other examples of goal-failure. If Fido, trapped in a cave-in, is in fact never reached, is it therefore false that he pawed at the door in order to be let out? If, as in the case previously described, the psychologist stops replacing the consumed pellets with new ones, is it false to say the rat continues to depress the lever in order to obtain a pellet? To suppose, with Braithwaite, that all teleological explanations carry with them an 'inference that the set of relevant conditions that will in fact occur in the future will fall within the variancy'¹ is to answer the preceding questions affirmatively and quite unplausibly.

¹ Ibid., p. 334

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The difficulty of goal-failure may, of course, be avoided if the prediction of goal-attainment is eliminated from the content of teleological descriptions. Retaining the rest of Braithwaite's scheme, we should then take such descriptions simply as attributing plasticity to the behaviour in question with respect to the goal indicated. We should be saying of the system b exhibiting the state e that there is more than one possible set of field conditions f , such that, were f conjoined with e , some suitable goal-attaining chain would ensue. We should not, however, be predicting that one of these sets will in fact be presently realised, and our statement would therefore be protected from falsification through goal-failure.

The Difficulty of Multiple Goals

This proposal is, however, immediately confronted with a new difficulty that we may call 'the difficulty of multiple goals'; the proposal becomes too inclusive to differentiate between acceptable and unacceptable teleological descriptions in an indefinitely large number of cases. These cases may be indicated schematically in Braithwaite's terms. Imagine that for the system b , and relative to the present state e of this system, the class of those sets of field conditions f uniquely determining gamma-goal-attaining chains has more than one member. The system in state e is thus plastic with respect to gamma. Suppose, now, that the class of those sets of field conditions f uniquely determining delta-goal-attaining chains relative to b and e also has more than one member. The same system in state e is thus also plastic with respect to delta. If our teleological description of the system's present behaviour embodies neither a prediction of the attainment of gamma nor a prediction of the attainment of delta but restricts itself merely to an assertion of the plasticity of this behaviour, we should be able, with equal warrant, to frame our teleological description either relative to gamma or relative to delta. We should, that is, be able to say either that b exhibits e in order to attain gamma or that b exhibits e in order to attain delta. This is, however, exactly what we cannot generally say.

The cat crouching before the vacant mouse-hole is crouching there in order to catch a mouse. Since no mouse is present, there will in fact be no goal-attainment. Nevertheless, the cat's behaviour is plastic since there are various hypothetical sets of field conditions, each set including one condition positing a mouse within the cat's range, such that, in conjunction with the cat's present behaviour, each

set determines a mouse-attaining causal chain. On the other hand, there are also various other hypothetical sets of field conditions, each set including one positing a bowl of cream within the cat's range, such that, conjoined to the cat's present behaviour, each set determines a cream-attaining causal chain. It should therefore be a matter of complete indifference, so far as the present proposal is concerned, whether we describe the cat as crouching before the mouse-hole in order to catch a mouse or as crouching before the mouse-hole in order to get some cream. The fact that we reject the latter teleological description while accepting the former is a fact that the present proposal cannot explain.

A limiting case of this difficulty occurs when the state e is one for which we should reject *every* positive teleological description. Imagine, for example, that e is the state of physical exhaustion and that the relevant class of field-condition sets is the one just considered in connection with mouse-attainment, suitably supplemented by conditions stipulating the cat's recuperation and assumption of a crouching position before the mouse-hole. The present proposal would then warrant us in saying, absurdly, that the cat is physically exhausted in order to catch a mouse.

Other examples of the difficulty of multiple goals arise in cases where every gamma-goal-attaining chain is also a delta-goal-attaining chain or contains such a chain as a part, so that if the variancy with respect to gamma has more than one member, so has the variancy with respect to delta. Consider, for example, an infant crying in order to get mother's cuddling, which is always preceded by the sound of mother's footsteps. Since every set of field conditions determining a cuddling chain in this situation also determines a footstep chain, and since we may assume the class of such sets determining cuddling chains to have more than one member, we may infer that the class of sets determining footstep chains also has more than one member. Thus we should again be warranted by the present proposal in saying either that the infant cries in order to receive its mother's cuddling or that it cries in order to hear mother's footsteps. The proposal again proves itself too inclusive to differentiate between teleological descriptions we accept and those we reject.

The Learning Suggestion again : a Caution

Can the wanted distinctions be made on the basis of the learning interpretation suggested earlier? The answer seems to be 'yes' with regard to the examples we have considered. Thus we do not believe

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the cat has learned to crouch before the mouse-hole as a result of having been rewarded with a bowl of cream for having done so in the past. Nor do we suppose the infant's crying to have been learned as a result of having heard its mother's footsteps following past crying intervals. Nevertheless, I *surely do not* wish to propose the learning interpretation as an adequate analysis simply on the basis of a few examples and I am convinced, moreover, that several lines of interpretation will be required to account for the variety of statements commonly labelled 'teleological'. Furthermore, to appeal to our beliefs as to how the behaviour in question has been learned is, I think, to provide no firm philosophical answer: it is rather to suggest the importance of addressing our critical attention to the field of learning.

2 *The Goal-Idea Strategy*

Background and Formulation

The second strategy mentioned at the outset consists, it will be recalled, in replacing reference to *goals* future to an action by reference to prior *ideas of such goals* functioning as causes of the action. What is the motive for this strategy? Suppose we explain John's decision to take a pre-medical course by saying that he decided to do so in order to enter medical school. Of what explanatory value is the apparent reference to John's future entrance? If it indeed comes to pass, it follows his present curricular decision and so cannot be among its causes. On the other hand, if it never comes to pass—for example, if John fails to complete his pre-medical training, we surely cannot count such entrance as a cause or partial cause of his actual decision. If the brick in fact did *not* strike the window, we cannot well say the window shattered because of the impact of the brick. What sense shall we then make of the fact that, although John's entrance to medical school is possibly fictional and, in any event, future to his decision, he clearly did choose his pre-medical course in order to enter medical school at a later date? The natural, and quite plausible, suggestion is to say that John's prior *desire* to enter, and his prior *belief* that entrance is contingent on choice of a pre-medical course, jointly determine his choice. Even if his desire is in the end thwarted and his belief in fact false, they are nevertheless realities of the situation preceding his choice, and they contribute, moreover, to its causal determination.

This strategy, quite plausible in the case just considered, has been rightly criticised in numerous others for projecting belief and desire into phenomena without adequate justification. As Ducasse has put it,

the disrepute into which teleological explanations have fallen is doubtless due to their having been so frequently thus put forth in cases where the existence of the agent appealed to and of his beliefs and desires was not already known, but invented outright and purely *ad hoc* . . . But when antecedent evidence for their existence is present (e.g., when the hypothetical agent is a human being), a teleological explanation is methodologically quite respectable, although, like any other, it may in a given case not happen to be the correct one.¹

Accordingly, Ducasse suggests the following as essential elements in genuine cases of purpose, rendering teleological explanation possible in the form exemplified by our recent illustration :

1. *Belief* by the performer of the act in a law . . . e.g. that if *X* occurs, *Y* occurs [or that *Y* is contingent upon *X*].
2. *Desire* by the performer that *Y* shall occur.
3. *Causation by that desire and that belief jointly*, of the performance of *X*.²

The sort of rule allowing explanation of the occurrence of *X* is further suggested by Ducasse as 'If an agent believes that *Y* is contingent upon *X* and desires *Y*, then that agent is likely to do *X*.'³

Now, even if we restrict the present strategy to clearly purposive human action in the sense specified by Ducasse, for example, we face serious problems in formulating the required explanatory statements. By way of illustration, let us try to explain John's curricular choice according to some such schema as that suggested by Ducasse. We ask, 'Why does John choose a pre-medical course?', and we receive the following statements in reply :

- (i) John desires John's entrance to medical school.
- (ii) John believes that John's entrance to medical school is contingent on John's choice of a pre-medical course.
- (iii) Whenever someone desires something, believing that it is contingent on something else, he performs this other thing.

In symbols :

(x) (y) (z) ((x desires y . x believes that y
contingent on z) $\supset$ x performs z),
(with the range of the variable 'z' restricted to acts or choices).

¹ C. J. Ducasse, 'Explanation, Mechanism, and Teleology', *Journal of Philosophy*, 1926, 23. Reprinted in Feigl and Sellars, *Readings in Philosophical Analysis*, New York, 1949, pp. 543-544.

² Ibid., p. 543. Addition in brackets mine; This seems quite in line with Ducasse's intent.

³ Ibid.

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This sample explanation is obviously crude and oversimplified, raising numerous questions peripheral to the logico-semantic difficulties that are my present concern. Thus, for example, to say 'John performs John's choice of a pre-medical course', in concluding our explanatory argument here, is indeed a violation of English but one that provides a uniform technique for describing action. More important is the fact that (iii) requires much qualification before it can be taken seriously as a truth of conduct. Nobody can perform every act upon which the fulfilment of his desires is believed to be contingent, for some such acts are beyond his power. Nor does anyone actually choose to do everything within his power that he believes necessary for the attainment of every one of his desires, even if the dictates of the latter are compatible. Nevertheless, if John's desire and belief are to figure in the causal account of his choice, as the strategy we are considering requires, some generalisation incorporating these three elements but considerably more complicated than (iii) must be supposed as a premise. Since any such generalisation faces the same difficulties that I wish to indicate with reference to (iii), the latter statement may here be taken as a simple model for a general problem.

Logico-ontological Difficulties : Objects of Desire

We may begin by asking, 'What is the intended range of the variable 'y' in (iii)?' Surely this range is not limited to actual things for, as mentioned earlier, the argument is intended to hold even if John's entrance to medical school never comes to pass. As in the case of purpose discussed earlier, we cannot infer existence of the objects of desire. To expand the range of 'y' to include possible but non-actual entities is not, moreover, sufficient, for the squaring of the circle and other impossibles have undoubtedly been desired. Allowing the variable 'y' to range over impossible so-called things as well is a step not many are prepared to take. It is bad enough to have to say, 'There is a possible but non-actual pot of gold at the end of the rainbow which has been desired by men', but to say, 'There exists a non-actual and, moreover, impossible squaring of the circle which has been desired by many' puts an impossible strain on the word 'exists', to say the very least.

Logico-ontological Difficulties : Belief

The variable 'y', further more, appears not only in the context ' x desires y', but also in the context ' x believes that y contingent on z ',

and both 'y' and 'z' refer back to prefixed quantifiers. Such quantification into belief contexts is, as Quine has repeatedly warned, a problematic procedure.¹ The occurrence of singular terms within belief contexts is, as he puts it, not purely referential in that the truth of such contexts taken as wholes depends not merely on the objects named by such terms but also on the manner of naming; belief contexts are, in short, referentially opaque. To use Quine's example, Philip may believe that Cicero denounced Catiline without believing that Tully denounced Catiline, though Cicero and Tully are identical. To replace either the name 'Cicero' or the name 'Tully' with a variable ranging over concrete objects such as people, in quantified belief sentences about Philip, thus yields an unintelligible result, since Philip's belief cannot be interpreted as a relation between Philip, Catiline, and some man to whom the names 'Cicero' and 'Tully' equally apply.

Quine has indeed admitted with reference to modal contexts, which are also referentially opaque, that a suitable limitation of the ontology to objects not nameable by non-synonymous names renders unrestricted quantification legitimate once more.² This result is due to the fact that interchangeability of synonymous names preserves truth value even within modal contexts, thus rendering the manner of naming again irrelevant and allowing modal statements to be true or false depending simply on the objects referred to. If, however, as I have elsewhere argued,³ synonymy is not sufficient to guarantee interchangeability with preservation of truth value within belief contexts, the ontological restriction above mentioned will not legitimise quantification into *belief* contexts despite its adequacy for *modal* contexts. To be safe, we should need to restrict our ontology to objects not nameable by non-identical names within our language. Instead of the concrete object Cicero, for example, we would now recognise as many objects in our new ontology as there are different names or descriptions of Cicero, whether synonymous or not. This seems a desperate expedient indeed, not to say a transparent projection of our language into the world of things.

¹ W. V. Quine, 'Notes on Existence and Necessity', *Journal of Philosophy*, 1943, 40, pp. 113ff.; Chap. VIII of *From a Logical Point of View*, Cambridge, Massachusetts, 1953; and 'Quantifiers and Propositional Attitudes', *Journal of Philosophy*, 1956, 53, pp. 177ff.

² W. V. Quine, *From a Logical Point of View*, pp. 150ff.

³ I. Scheffler, 'On Synonymy and Indirect Discourse', *Philosophy of Science*, 1955, 22, pp. 39ff

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Notice, finally, that the variable 'z' occurs once within the belief context and once outside it, subject to the same external quantifier, thus serving to tie together the agent's act with his view of his act, as the variable 'y' serves to tie together the agent's desire and his beliefs about the conditions of fulfilment of that desire. Waiving all questions as to the nature and individuation of such entities as acts, it should be noted that these connections between act, desire, and belief are essential not only to the strategy under consideration, but also to every developed conception of rational behaviour. Indeed, if we have no way of formulating these connections in such a way as to avoid the difficulties discussed, we lack a satisfactory account of human action. The following remarks are devoted to the exploration of a possible solution.

Suggestion for an inscriptional interpretation

Suppose we try to construe desire, belief, and performance, at least with respect to the present schema, as relations between agents and inscriptions. The initial advantage to be hoped for is, of course, the relative clarity of inscriptions as concrete physical objects in comparison with the possibles, impossibles, and other strange entities posited by the approaches we have so far considered. The existence of inscriptions is, furthermore, completely independent of the existence of their purported objects. Though no pot of gold dangles from the end of any rainbow, we cannot infer that no inscription affirms that there is such a pot of gold. Where an object of desire fails to exist, its purported description need not, correspondingly, fail to exist. Even where something lacks not only being but possibility, its non-existence does not generally carry over to its purported description. To assert the existence of something both square and not square is a serious and offensive thing to do; to attribute being to some inscription asserting such existence is something else and quite innocuous. Can we reconstrue the explanatory argument (i)-(iii) in such a way as to capitalise on the advantages of an inscriptional formulation?

Desiring True

Suppose we begin by transforming (i) into the following statement, converting the singular term naming the object of John's desire into a clause:

(I) John desires that John enter medical school.

Let us now construe this statement as:

(i') John desires-true some that-John-enters-medical-school inscription.

Desiring-true is here to be taken as a relation between people or other suitable agents on the one hand and inscriptions on the other, and does not require that the agent produce or even understand the inscription that he desires-true. To interpret this first premise of the explanatory argument as we have done can, moreover, be no more obscure than to construe it as relating John to some such entity as a hypothetical state of affairs named by the *that*-clause of the premise. For it is always possible to satisfy proponents of the latter construal by explaining the desiring-true of a given inscription as the desiring of that state-of-affairs which is named by it or by the proposition it expresses. The present interpretation, it should also be noted, treats the *that*-clause of (I) as a single predicate that applies to inscriptions peculiarly related to itself—inscriptions constituting rephrasals of its own sentential content, *including this very content as well*.¹ Thus (i') tells us that John desires-true something which is a *that*-John-enters-medical-school. Every such thing, further, is an inscription rephrasing the 'John enters medical school'-inscription that forms the content of the *that*-clause predicate of (i'). We shall, finally, make the assumption that to desire-true any given inscription denoted by a *that*-clause predicate is to desire-true every such inscription.

Believing True

Turning now to the second premise (ii) of our explanatory argument, let us first transform it into the following statement, eliminating reference to contingency in favour of a conditional of the type suggested by Ducasse :

- (II) John believes that if John enters medical school then John chooses a pre-medical course.

Let us now interpret this statement as :

- (ii') John believes-true some *that-if-John-enters-medical-school-then-John-chooses-a-pre-medical-course* inscription.

*Believing-true*² is, analogously with *desiring-true*, here taken to be a relation between agents and inscriptions, not implying that the agent

¹ See I. Scheffler, 'An Inscriptional Approach to Indirect Quotation', *Analysis*, 1957, 14, pp. 83ff., for a fuller explanation of this general approach. The fact that the sentential content of the *that*-clause predicate is included in its application guarantees the existence of an appropriate inscription by the very existence of the stated premise.

² This relation is suggested by W. V. Quine, 'Quantifiers and Propositional Attitudes', p. 186. Quine uses *believing-true* as a relation between agents and sentences, and thus expands it from a dyadic to a triadic relation, relativising it to language.

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produce or understand those inscriptions he believes-true. Such an interpretation is at least as clear as the usual construal of belief as a relation between a person and a proposition, for the believing-true of a given inscription is explicable to the intensionalist as the believing of the proposition expressed by it or by the statement it designates. The that-clause of (II) is to be treated also as a single unanalysable predicate applicable to rephrasals of its own sentential content, *inclusive of such content*.¹ Finally, we are to assume that to believe-true some inscription denoted by a given that-clause predicate is to believe-true every inscription denoted by that predicate.

Making True

Turning now to the third premise required to complete our explanatory argument, let us replace the original (iii) with the following :

- (iii') Whenever anyone x desires-true any inscription v and believes-true another inscription z , such that z is the conditional of v and a third inscription w , then x makes-true w . (Here the range of the variable ' w ' is restricted to English sentence-inscriptions of the form ' x chooses. . .'.)²

Making-true, similarly to desiring-true and believing-true, is a relation between agents and inscriptions and does not require the agent to produce or even to understand the inscriptions he makes-true. Making-true a given inscription can always, furthermore, be explained as performing the act described by the inscription or by its associated proposition ; in the present case, John's making-true of a 'John chooses a pre-medical course'-inscription is explicable as his performing of the choice described, as his doing of the act. We shall, further, suppose that to make-true a given inscription is to make-true every rephrasal of it. It should especially be noted that, unlike inscriptions desired-true or believed-true, every inscription made-true is in fact true, together with all its rephrasals.

Reformulation of the Teleological Argument

How shall we now explain John's choice of a pre-medical course with the aid of our reformulated premises ? From (i') we learn that

¹ As with desiring-true, the existence of an appropriate inscription is guaranteed by the existence of the whole premise.

² Strictly, other restrictions on this range would be needed to prevent awkward results. It might be best, for example, to restrict the vocabulary and syntax of such inscriptions in certain ways. But I am concentrating on the general idea here and so do not enter into details.

John desires-true some inscription x to which the that-clause predicate of (i') is applicable. This inscription x , we know, is a rephrasal of the content of this predicate itself. (ii') conveys that John believes-true some inscription y denoted by the that-clause predicate of (ii'). Looking at this latter predicate of (ii'), we note that its sentential content is a conditional of which the antecedent is a *replica* of the content of the that-clause predicate of (i'). On the assumption that (i') and (ii') belong to the same language and contain no indicator terms, this antecedent must therefore also be a *rephrasal* of the that-clause content of (i'). If so, it must also be denoted by the that-clause predicate of (i'), that is, this antecedent must also be a that-John-enters-medical-school. Since, moreover, (i') says that John desires-true *some* inscription x which is a that-John-enters-medical-school, he must also desire-true the antecedent in question. This follows because of our assumption that to desire-true any inscription denoted by a given that-clause predicate is to desire-true every such inscription.

Now y , which John believes-true according to (ii'), is denoted by its that-clause predicate. But we know that the content of this predicate itself is also so denoted. Thus we may conclude that John believes-true this very content itself, i.e. the conditional inscription of (ii'). This conclusion, it will be recalled, is warranted by our assumption that to believe-true any inscription denoted by a given that-clause predicate is to believe-true every inscription denoted by that predicate.

The conditional inscription of (ii') believed-true by John is, however, the identical one of which we already know that John desires-true the antecedent. This conditional may thus be taken as the conditional z of (iii'), its antecedent may be taken as v in (iii'), and its consequent may then be taken as w of (iii'). The conclusion may now be drawn that John makes-true the consequent of the conditional in question as well as every one of its rephrasals. Since the consequent is made-true, it is in fact true, together with every one of its rephrasals. We may now, in particular, write down a replica of this consequent in the same language as our three premises. On the assumption that it contains no indicators, it will then also be a rephrasal of the consequent, hence true. It will thus represent the needed conclusion of our argument :

(iv') John chooses a pre-medical course.

The foregoing analysis is only exploratory. It remains to be seen to what extent the treatment of our example can be generalised. If it proves sufficiently general, it will, by avoiding reference to desired

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states-of-affairs, believed propositions, and performed acts, have managed also to escape the semantical difficulties plaguing the second strategy we have considered. It does not, however, provide an 'operational' definition of 'belief', 'desire', and 'performance' idioms; thus believing-true, desiring-true, and making-true are, for all that we have said, no clearer *in application* than these original idioms themselves. Their explication presents an important problem left unsolved by our above treatment, which has addressed itself to specific semantic difficulties.

Summary

1. Two strategies for interpreting teleology have been examined: the self-regulation strategy, and the goal-idea strategy. Criticism has been directed explicitly against the moderate use of each strategy, i.e. the use of the first solely for non-purposive behaviour and the use of the second solely for purposive behaviour.

2. Two basic variants of the self-regulation strategy have been criticised as inadequate to account for non-purposive (let alone purposive) teleological behaviour. (A) The inadequacy of the 'negative feedback' variant has been discussed in connection with the difficulty of the missing goal-object. (B) The inadequacy of the 'plasticity' variant has been argued by reference to the difficulty of goal-failure and the difficulty of multiple goals. (C) These inadequacies are not, of course, taken as proving that the self-regulation strategy is never applicable, but only as showing that it is not sufficient, even for non-purposive teleological behaviour.

3. With respect to non-purposive cases, another interpretation of teleology, in terms of learning, is suggested. (A) This interpretation construes the future-reference of teleological statements not as predictive nor as descriptive of objects in the current environment, but as pointing to the rôle of consequences in the genesis of the learned behaviour in question. (B) The learning interpretation seems to avoid the difficulties previously discussed, but the matter both requires and deserves further study. (C) The learning interpretation is, in any event, proposed as only *one* among several that are likely to be needed.

4. The second (i.e. goal-idea) strategy has been argued to involve logico-ontological difficulties in its natural formulation, which makes use of the notions of belief, desire, and performance. These difficulties hold even when the strategy is applied to clearly purposive cases.

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5. An inscriptional proposal for avoiding these difficulties is explored. (A) This proposal makes use of the new notions 'desiring-true', 'believing-true', and 'making-true', holding between agents and inscriptions. (B) A sample teleological explanation is reformulated in terms of these notions, to show how the argument may be carried through in a way that is both materially adequate and free of the logico-ontological difficulties mentioned earlier. (C) This proposal, too, requires further study, and is, in any event, not intended to give an *explication* or operational analysis of belief, desire, and performance.

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TIME AND THOUGHT *

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I *Two Views of Time*

ARE there, I would first of all ask, two utterly different views of time, the outside view and the inside view? What I mean by the outside view is illustrated especially by the ways of thought of academic people in their academic capacity. The mathematician and the historian are the examples I have particularly in mind. The mathematician treats time as a space, or as one dimension of a space, in which all points have an equal status or importance or validity *together*, within one and the same prospect of the world; they have, as I would paradoxically say, a *simultaneous* validity, each of them means the same to him when he thinks about them all in one thought. This attitude of the mathematician is brought to a focus when we consider what is implied by his writing down a differential equation to express, say, the motion of the 'particle' of classical dynamics. In this equation, which, at each moment when he contemplates it, he regards as valid, he has implicitly

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¹ This paper stands as it was written in the winter of 1957-58, as one more result of the kindness and encouragement of Professor Burton Keirstead. Since completing it, I have had the marvellous intellectual experience of reading Professor Michael Polanyi's *Personal Knowledge*. This book, whose passages of luminous beauty give it rank as literature no less than as science, provides an epistemic setting within which, perhaps, the difficulties that I have tried in this paper to define might find solutions and lead to constructive development. To Professor Karl Popper I owe an early impulse to attack these questions. The excitement and ferment of thought that I carried away from a lecture of his in 1936 live with me still. He has now with very great kindness written out a detailed criticism of the views here expressed, and some radical differences between us will act as a fresh incentive to thought. Professor Keirstead himself takes issue with me in a forthcoming article in *Metroeconomica*, with delicacy and subtlety that are, perhaps, too kind. Amongst such relevant work as I have read in earlier years Professor W. B. Gallie's masterly book on Charles Peirce is outstanding. Mr J. W. N. Watkins by his perfect understanding of what I was driving at in earlier work, and now Professor Johan Åkerman in a just-published article of searching insight in *Kyklos*, have greatly improved my grasp of my own ideas. To each of them I am very deeply grateful. All my efforts for eight or nine years past have drawn strength from Professor C. F. Carter's critical support.

stated the position which the particle will, or did, or would, occupy at each instant during some stretch of time. The equation cannot exist and have meaning, therefore, unless all those instants can be looked on as having an equal validity *together*. In the mathematician's act of writing down his equation, all the instants and the corresponding positions of the particle are folded up into one indivisible statement. At which of these instants does he place himself, at which does he take his stand? At none of them, he is an *outside observer*, not part of the system he is describing, and for him all the instants are, in the instantaneous logic of his own thought, equally and simultaneously valid and meaningful.

Consider the historian who is thinking, say, about the constitutional changes produced in England by the war between king and parliament in the seventeenth century. Can he at that moment help looking back, half unconsciously perhaps, at the beginnings of political freedom in England, at Magna Carta, at Simon de Montfort; and looking 'forward' to the great reforms of the nineteenth century, and on to the beginnings of the Welfare State in Asquith's government of 1911? All this long process presents itself to him in one panorama, as a unity, every part of it as real as every other part; he is an outside observer, not himself part, of what he describes.

With this outside, detached, sophisticated view of time, I want to contrast the *inside* view which each of us has in the very act of living, the time *in* which we sense-perceive, feel, think, imagine, and decide. From this inside view, the time of our actual psychic experience is but a moment, utterly solitary in its isolation from all other moments. It is what I would like to call the *solitary present* or the *moment-in-being*. The experienced moment, the moment-in-being, is for the individual person the only thing there is, the only actuality. I do not mean that it is the only *real* thing: he may believe in the reality of things outside his own sense perceptions and psychic processes. Let me put the contrast, as I see it, between the outside and the inside view of time in another way. Time from the inside is the time *in* which we think, time from the outside is the time about which we think. Thus I want to attach a rather special meaning to the word *actual*, the meaning of presence and pressure upon the mind, the fleshly existence of events, as distinct from the content of those events that pass upon the mental screen of memory and imagination. I would say, then, that for any one person, no two distinct moments can be actual together, the actuality of one denies and excludes the actuality of any other.

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In what sense, then, can the *actual* experiences of two distinct moments be compared? In no sense at all, such comparison is meaningless and impossible, for there is no 'common ground', no meeting place for two mutually destructive actualities. Within the actual present, of course, different elements of the picture can be compared. These elements include, on the one hand, the sense-impressions of what is going on 'now' and also, I would say, that sort of neurological expectation, that forward-feeling of our nervous system which links one moment to the next; and on the other hand, those things which the mind creates, as it were, unaided: memories and *experiences by imagination*. Memory and imagination are both of them part of a person's present experience, they belong to the essence of the moment-in-being, they are in it and of it. The content or subject-matter of their images does, indeed, bear a label with a date other than that of the moment-in-being, but this fact no more allows us to treat these images as effective *substitutes* for the actuality of other moments than the presence in a painting of the images of other places enables us to treat the painting as a full substitute for those other places.

2 *Dynamic and Imaginary Movement in Time*

The moment-in-being must surely be looked on as something which exists only by changing. It is a unity, a self-interlocking whole, yet I suggest we can better use the analogy of a wave rather than that of a particle. Can a wave be identified? There is, presumably, a certain balance and mutual dependence between the parts of a wave, the crest and the trough, which entitle and require it to be thought of as a unity, yet we can 'cut off' the segment at any two suitably related points, at the bottoms of two troughs or at the tops of two crests. Any such analogy is crude in the extreme, perhaps scarcely worth invoking. Yet it seems vital to distinguish between two ways in which the moment-in-being, time from the inside, is related to the calendar axis, time from the outside.

By the phrase *dynamic movement in time* I refer to the notion, first, that the moment-in-being, even from the outside view, is not a bead upon a string but rather a colour of a spectrum, distinguishable as something in itself yet overlapping and growing out of and into other colours, so that while it is possible to name a particular wave-length of light, say 0.0000 metres, yet such a wave-length is not the whole of a discernible colour but rather corresponds to an *instant* or point of time.

than a moment; and secondly, that the moment is the locus of events, that is, of sensations, thoughts and acts, which 'take time' and whose very nature, intimately bound up with that of the moment, is such that by merely taking place they carry us into a different moment. Thus by dynamic movement in time I mean the translation of the moment-in-being along the calendar axis; that is the outside view. Or I mean the transformation of one moment into another; that is the inside view. There is, however, another thing that we might call 'movement in time', and this other thing is indeed my central theme. It consists in the mind's power to create images unaided by outside stimulus, and to label them with dates other than the actual date at which such images are created.

Imagination, I would say, is of three kinds. It can be, firstly, the free unlimited fantasy of day-dreams. Regarding this it is irrelevant to ask whether what is imagined could come true. Secondly, there is the imagination which constrains its images in two ways, making them in the first place compatible with the individual's beliefs about the nature of things and about human nature, so that they represent something that seems to him possible in the abstract, and in the second place attaching them to named future dates and restricting them to such transformations of his existing situation as seem to him possible in the time-span between 'now' and 'then'. Such images are thus subjectively possible consequences of action-schemes open to the individual, and I shall call them *expectations*. Thirdly when, amongst all the courses of action open to him, the individual has chosen one and mentally committed himself to it, constrained imagination becomes *anticipation*, an experiencing beforehand of some supposed outcome of a decision.

Imagination in the two latter of these senses might seem to provide a substitute for the actuality of moments other than the individual's moment in being. Yet surely this is a delusion. Actuality is unique, expectation is multiple and uncertain. As the individual's viewpoint advances, as his moment-in-being transforms itself, the relative vividness of the rival expectational pictures which he forms of the outcome of any one action-scheme shifts repeatedly with the impact of news and the continuous process of interpreting it. How can this be a substitute for the uniquely certain actuality of a moment-in-being? If we are still tempted to think that two actualities can exist together and be brought face to face in the same moment of thought let us suppose that the action-scheme which a man has decided on affords

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him by anticipation both pleasures and pains. The pleasures are associated in his mind with a calendar label t_1 and the pains with a later calendar label t_3 . As he did decide on this action-scheme there was plainly some date, t_0 , when the good anticipations outweighed the bad. But what are his feelings at a date t_2 , part-way between t_1 and t_3 ? There is now nothing pleasurable, we are assuming, for him to look forward to amongst the consequences of the action-scheme but only its pains that are the price, still unpaid, of the pleasures which now belong to memory. Is it not possible that in this moment he will regret his decision? It is not certain he will, for memory has pleasures of its own. But if, as outside observers, we see him regretting at t_2 the action-course that, at t_0 , had seemed preferable to all others, we may want to ask: Which of these two views, taken at different dates by the same man about the same act, is just? I contend that this question is meaningless. There is no co-existence of these two moments, t_0 which contains as part of itself the decision to adopt the action-scheme, and t_2 which contains as part of itself the regret for that course. These two moments cannot, for the individual to whom alone the question could matter, be 'in being' together, the actuality of one denies and excludes the actuality of the other; and it is only by assuming ourselves to be observers standing *outside* of the system under discussion, the thought-system of this individual, and outside of the time, the moment, in which his thoughts take place, that we can make the question even seem to mean something.

If it be true that the *actualities* of two distinct dates cannot co-exist, it follows that choice between rival action-schemes must surely be based on comparison of their expected consequences. It is accordingly experiences by imagination, subjective entities, which guide decision. We cannot look upon decision as depending on a subjective evaluation of objective realities: it is subjective from the start. If the act of deciding is the act of maximising something, that something must be a feeling belonging to the same moment as the act of decision itself. The purpose, the desired effect, of decision will be experience by *anticipation*, that is, by imagination limited to the seemingly possible, and this act of imagination will be part of the moment of decision.

What is left of the distinction between rational and irrational conduct? Conduct is judged, by an outside observer, to have been rational when it has brought consequences which, at the moment of experiencing them, the individual finds superior to any others which he now judges he could have secured. Does this mean that, in any

other case, his conduct at some earlier date was 'mistaken'? By what right does the later moment claim jurisdiction over the earlier? The earlier had its own enjoyments by anticipation towards which, in their actuality at the moment of decision, the later moment can contribute nothing and from which its conflicting evaluation has the right to detract nothing. The two moments are eternally exclusive of each other and wholly incomparable. Rationality means something only for the outside observer. Those who claim that, in choosing an action-scheme in face of a not-to-be-repeated set of circumstances, a decision-maker would do himself most good by applying a calculus of 'objective' probabilities to the rival claims of various imagined outcomes of each action, are looking at time from the outside and making all the dates 'co-actual'. Would do himself most good, *when*?

3 *The Nature of Decision*

Either of two suppositions would destroy the meaning with which our habits of thought and speech seem to invest the word *decision*. If the transformation of the moment-in-being as it reaches point after point of the calendar axis is no more than the rolling of a wheel along a road which constantly and fixedly exists in its entirety wherever the wheel is located, so that the human consciousness merely comes across situations, or the events which carry one situation into another, as inevitably as the traveller comes across the mountains and oceans that lie in his path, then human decisions are mere acts of recognition of the compulsion of circumstance. If we regard the history of the world and of each person in it as the unrolling of an initially complete design, as determinate and pre-destinate, then 'decisions' can play no more vital and spontaneous role in this process than the shapes of the pieces of a jig-saw puzzle play in the fitting together of the puzzle. The final picture takes its character from the necessity of fitting the pieces together in just the one possible way; yet the profile of each piece was cut before ever the fitting together began to be attempted; the character of the ultimate picture was pre-determined, and it would be against all sense to ascribe what gradually emerges as being created by the profiles. Yet we speak and think as though decisions were *creative*. Our spontaneous and intuitive habits of mind treat a decision as introducing a new strand into the tapestry as it is woven, as injecting into the dance and play of events something *essentially* new, something not implicit in what had gone before. The two

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conceptions are totally opposed. Either decision is nothing or, it is everything.

A supposition virtually equivalent to that of determinism is perfect knowledge. If a person felt himself to have in mind a complete specification of the entire set or range of action-schemes open to him, and for each of these action-schemes, a complete and certain knowledge of its entire consequences as far as these were of interest to himself, and if also he could order these outcomes, one for each action-scheme, according to his preferences; then his selection of one action-scheme out of all those open would, we should surely have to say, be mechanical and inevitable, and would have no content of the kind which makes our *working* ideas about decision interesting. Decision would be *empty*, the mere registering of a formal solution to a purely formal problem.

Exact, complete, and certain knowledge of what would follow from any possible act, utterly remote as this notion is from experienced life, is the tacit basis of much of economic theory. Yet when this strand is relinquished, it is exchanged only for the most worthless substitute, the assumption that, even if the individual cannot pick a unique train of consequence for each possible act, he can make a list, complete and, somehow, known to him to be complete, of all the possible trains, and attach to each of them a proper fraction giving the proportion of times, in an infinitely long lifetime of repetitive experiment, that this outcome would prove the true one. I do not want at this moment to consider the second half of this assumption, the invoking of notions of relative frequency. That is a separate matter. The true basis of the disaster wrought upon economic theory by the *games of chance* universe of ideas is the notion of the existence and the attainability of a list, complete and known to be complete, of all the possible outcomes of an action. In games of chance this possibility, of listing completely all the contingencies, is assured by the very nature of these games, their inherent and essential dependence upon a set of explicit rules. It is the completeness of this list which makes it logically possible to distribute relative frequencies over the contingencies. Once we abandon the notion of rules of an artificial game and the 'objective' list of its possible states, and mean by 'possible' outcome of an act anything that the individual thinks to be possible, we find that outcomes are not *given* to the individual but imaginatively *created* by him. And who is to predict, and how, what outcomes will be imagined by the individual for each possible one of

his acts? Who is to say when he has finished his work of creating his list of possible outcomes? In what sense can such a list, even in the individual's own reckoning, ever be complete?

The word *decision* surely epitomises part of our intuitive, spontaneous attitude to life, and an attitude which appears more surprising the more attentively it is compared with that other attitude which has been conditioned into us by education, our scientific attitude to life. That spontaneous and natural attitude is, I suppose, common to all of us, so our habits of speech suggest; and it is surprising only when we look at it against the background of the characteristic scientific attitude. Between man in his spontaneous naturalness, and man in his superior scientific wisdom, there is a deep divorce and alienation. Natural man believes, without thought or question, in his own creativity, in his power to conjure from the air a new strand in the texture of affairs, by merely saying *I decide*. . . . Scientific man says that he can predict the course of events. Give him an adequate description of what has been up to now, and he will tell you what will be after this. Now these two decisions are utterly at variance. One cannot suppose that the man who claims to predict allows to himself, but to no-one else, the inceptive power of genuine decision, the capacity to inject something *essentially* new into the whirlpool of events; and one cannot suppose that he believes it logically possible to foresee his own decisions while still looking upon them as decisions, as future events which will be creative in the full sense. Yet if he does not make these claims, how does he suppose himself able to predict? Someone else's decision, or even his own, could blow his prediction to the winds.

4 *The World of Order and Inspiration*

If a man felt that he knew completely and for certain all the consequences, of any practical or emotional concern to him, that would flow from each given act in the range of acts open to him; and if he felt that he could rank these various trains of consequences, one for each act, according to his preference; then for him the act of choosing between these acts would be purely formal and automatic, his 'decision' would be empty. If we opt for this assumption there is nothing in the idea of decision worthy of being discussed. All we need is an analysis of 'economic forces', a theory of the economic system fully as independent of any specific psychic or humane elements as the theory of the solar system is.

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But if not, what must we mean by decision? If it must not be choice under perfect foresight, it must be choice in face of doubt and ignorance. Yet it cannot be choice in face of chaos and anarchy; for a man who thought that any act could have any sequel whatever, and that there was no possibility of excluding *anything* as incapable of following from any stated course of action, would believe any one act to be just as eligible, just as wise and efficient, as any other, and decision would be pointless. Decision can only mean what we ordinarily do mean by it, it can only be non-empty and non-futile, in a world of bounded uncertainty. Let me then state my definition: Decision is choice in face of bounded uncertainty.

The world of pure determinism where 'decisions' are the mere clicking of the machine as it works, the world of perfect foresight where everything is actual at once and all possible experiences are in effect pre-experienced in one comprehensive explosion of events, and the world of anarchy where nothing that happens is constrained in any degree by what went before nor itself constrains what will come after, are alike fatal to that content of the word that our whole attitude to life assumes, the implicit belief that a decision is in some strict and full sense a beginning, something constrained indeed but not determined. In order to study decision in this sense, we must assume a world which satisfies two conditions. First, it must be a world where natural law operates, where a man feels that if his immediate act were different the sequel would be in some respects different, where, in fact, the course of the game can be influenced and affected, but not determined, by the individual player: in short, a world of order. But within this order, secondly, there must be room for the creative process to be still at work. For we have, it seems, the choice between two assumptions. Either creation was a single act and all that comes after was thereby settled and fixed forever; or creation is a continuing process and the shape of events can still arise from the void through men's inspired decisions. We have to suppose that into each moment in each man's mind there can be injected something *essentially* new, something not arising, in its completeness and its essence, merely as the one inevitable response to the impact of a given stimulus on the existing stock of his mind, nor as the only possible upshot of the inner working of that existing stock of thoughts, ideas, and knowledge.

In short, whether or not there is such a thing as *decision* in the full sense that our spontaneous working habits of mind give to this word,

such decision if it does exist belongs only to a certain scheme of thought about the world, that which supposes in it *order and inspiration*.

5 Prediction

If, as the basis of our thinking, we opt for a world of non-empty decisions, we are denying the possibility of exact, unlimited prediction of human affairs. For a decision, we are saying, is a cut in the logical connectedness of past and future. We are saying that part of what enters into a decision is uncaused. Are decisions creative or predictable? They cannot be both, for the meanings of the two words are mutually exclusive. Thus prediction will have to mean, for us, something other than exact calculation. But the idea that, in principle, the entire conduct of every individual, and of all individuals together, could be predicted, if we knew all there is to know about what has happened up to 'the present', conflicts both with our experience of time from the 'inside', as an evolving, solitary moment, and with our view of time from the 'outside', as an extension, an axis on which a plurality, indeed an infinity, of separated points can be distinguished. For to have perfect foreknowledge of the future would surely be to pre-experience the future, to telescope all of life into a moment. It is hard to regard this even as an idea, so plainly does it conflict with the nature of consciousness itself. But perfect foresight appears even to involve logical self-contradictions. What could be meant by perfect foresight of an invention or discovery? This is a contradiction in terms. We might say the same of the idea of perfect foresight of a decision of one's own, were it not that the concept of non-empty decision is in any case not meaningful in a deterministic world. The logical absurdity involved in supposing two people both to have perfect foresight of each other's free actions was happily shown by Oskar Morgenstern with the help of Sherlock Holmes. If the escaping criminal, Professor Moriarty, had perfectly foreseen the course of Holmes's pursuit, he could have evaded it; but if Holmes had perfectly foreseen this evasive action, he would have defeated it. The self-contradiction of the idea that several people can all use perfect foresight in pursuit of rival ends closely parallels the dilemma pointed out by Popper, that some predictions, if made public and believed, would induce action tending to falsify them. We could suppose the predictor to use his power of foreknowledge merely to choose for himself whatever course of action would most benefit him, keeping this knowledge and his own

intentions secret from others. But why should we suppose that the system contained just one and only one person with this power? It could not contain several, for their respective powers of foresight would mutually cancel each other. The only possible predictor whom we could suppose to escape any danger of causing the falsification of his own predictions would be an observer entirely outside and detached from the system. And it is, indeed, only for such a person that the idea of an economic dynamics in the orthodox sense, a scheme of thought embracing a stretch of historic time and treating it as all actual at once, could have any meaning.

6 *Can We have an Economic Dynamics ?*

Economic dynamics can be comprehensively defined only as everything that is not the analysis of a system abstracted from every meaning of time. The pure static system is one where either there are no changes, or where all changes take place instantaneously, so that all *connected* changes take place simultaneously. Nothing short of this will really do. The stationary state is a mere concession to intellectual weakness. What can an infinite repetition of moments, all identical with each other, and each perfectly self-contained in the sense that all that happens within it exhausts its effect within the self-same moment, teach us, that a single one of these moments cannot teach? You may object that in the stationary state we can suppose today's work to produce tomorrow's saleable goods, so that there is a certain temporal interlocking without change. But what is the good of such a supposition? So long as there is no change, we might just as well suppose every moment to be self-contained. The stationary state is at best as artificial as the static system, since while the latter abolishes expectation altogether, the former constrains it to such beliefs about the future as can be entertained without giving rise to change. The static system is clear-cut and goes the whole way to exclude time, the stationary state only pretends to admit it.

It would be natural now to ask whether our departure from the static system, in order to obtain an economic dynamics, should lead us to the inside or the outside view of time. I would answer that there are two conceivable classes of economic dynamics, one corresponding to each of the two views of time. It is plainly the outside view which gives us an economic dynamics in the accepted and orthodox sense. For in that sense we consider a sequence of moments to belong to one

and the same actuality, and that actuality must therefore be that which is seen from the outside by the eye of an observer who is himself no part of the system; were he to include himself in his view of it, all moments except the solitary present would lose their actuality. This exterior dynamics falls further into three kinds. In its strict and calculable form the current values of all variables are made to depend on some set of their previous values, so that, if over some sufficient interval or series of dates the values of a self-contained set of variables are known, all subsequent values can be calculated. In such a dynamics there can of course be decision only in the empty sense. Inspired decision, true decision in our sense, is, precisely, an unpredictable initiative. But there is an inertia in affairs. However far a decision may depart from being an obvious reflection of obvious circumstance, its effect will take time to work through the economy, which meanwhile will swing along a path at first largely shaped by its antecedent states. It is, perhaps, only a relatively few key individuals at any time whose decisions can singly contribute to this course anything that can ever be imputed to them by our outside observer, though we must suppose, perhaps, that any man's decision can set off a chain reaction that will amount to a great effect. Nevertheless we are supposing each man to make his decisions in a world of subjectively bounded uncertainty; for each action open to him he discerns a great range of possible ultimate consequences, but a range which, within any finite horizon, is not utterly unlimited. All this may give to the sequence of states seen by our detached observer a sort of continuity of texture which will enable him without absurdity to make short-range guesses. The basis of such guesses is the second kind of exterior dynamics. Thirdly we can in a certain manner combine the first two kinds into what I would call a *ceteris paribus* dynamics. We assume like the Swedish period analysis, that at a particular instant a number of economic agents all take decisions simultaneously, and that thereafter during a certain interval everything that happens is the direct or indirect consequence of these initial decisions. By a direct consequence I mean here an act which precisely executes the initial stages of a decision, and by an indirect consequence I mean an act which is the 'automatic' response merely to events which themselves spring directly from the initial decisions, or from the interplay of acts directly arising from those decisions, without any new decisions in the non-empty sense. In this manner we can at one and the same time bring fully into our analysis the inseverable structure of expectations

and decisions and yet allow ourselves to trace consequences from antecedents on the supposition that no *essentially* new initiative interferes.

Let me turn to the *interior dynamics* of the individual's solitary moment-in-being. It can be constructed only by each person for himself, since he alone can have insight into his own mind. For this reason many will reject the notion of an interior dynamics as unscientific. Extrasppection is scientific, introspection is not; sense-perception is respectable, self-perception is delusory. Why? Descartes at any rate did not agree. He based his belief in his own reality on the fact of his own thoughts: *Cogito, ergo sum*.

The psychic solitary moment I would suppose to consist in the creation and use of expectations; to consist in imaginative creation of the set of possible action-schemes and, for each action-scheme, a bounded range of its possible outcomes; in the focusing of attention on certain *expectation-elements* of each such bounded range; and the selection of one action-scheme out of all those open, whose focus-elements of expectation will serve as the basis of anticipatory experience. I must apologise here for having had to bring in a reference to arguments and ideas which there is no space to develop in this paper.

Between an exterior and an interior dynamics there are plainly many essential differences of nature. An exterior dynamics is *public* and *objective*: the thing studied by one outside observer can be studied by another, since it exists in some sense independently of the existence of any observer. It is *mechanical*, for it looks upon each momentary state of the system as a phase in the determinate behaviour of a *machine of limited design*, a machine whose whole potentialities we can in principle know, so as to be able to tell, from information about what has happened up to now, what will happen next. And an exterior dynamics will be *aggregative*, for it deals with the totality of the actions of many individuals to each of whom the observer's own relation is the same: that of aloof and detached study. And above all, the exterior dynamics will claim to be *predictive*. In its calculable form an exterior dynamics is in a certain sense, paradoxically, timeless. The time which it considers is denatured, its vital principle of creative uncertainty, which belongs only to interior time, has been wrung out of it, so that all its infinity of distinct points along the calendar-axis are, to the outside observer, all equally actual and their 'content' or events all equally known. Exterior dynamics of our second kind, which I would like to call inertial dynamics, is by its whole purpose predictive,

though in a tentative and undogmatic way. The writer of 'inertial' dynamics invites his readers to watch for departures from the inertial course of events, he continually suggests what could happen if this and if that unpredictable impulse should strike the system from outside its defined boundaries of internal interdependence. In this use of the word *if*, he is moving away already from the role of prophet towards the task of scientific *description*. To describe the orderliness of nature: this is all that it lies within the power of the scientist, as such, to do. In this light, economic statics and economic dynamics of any and every kind are one: a means of description merely.

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P. W. BRIDGMAN'S OPERATIONAL ANALYSIS: THE DIFFERENTIAL ASPECT *

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Introduction

P. W. BRIDGMAN's philosophy of science, the celebrated Operationalism, has become widely known in the last few decades and has been frequently the subject of discussion amongst philosophers and scientists. Some have hailed it as epoch making. Others have not seen in it much that was new and had not already been said by empiricists during the last few centuries, while yet others have considered his views not only erroneous but even harmful to the progress of science if accepted and insisted upon.

There can be no doubt that both friendly and hostile commentators have often misrepresented his views. The chief reason for this is perhaps the fact that his commentators have derived their ideas about Operationalism mostly from general expositions endeavouring to clarify its meaning in abstract terms, instead of following his applications of Operational Analysis to actual examples. Bridgman himself realises the inadequacy of his own exposition:

It would be difficult to exhaustively characterise the operational technique of analysis; it is more of the nature of an art to be learned by the practice of it and by observation of its practice by others.¹

Therefore it is better not to pay too much attention to what has been said about Bridgman's ideas, either by others, or even by Bridgman himself. Far more insight will be gained by following these views in their application to actual physical cases. Many of the unexpressed qualifications and restrictions that become apparent in the light of practical analysis show Operationalism to be a far less concise and clearcut system than it appears at first sight. Yet in its more modest and practical form it contains much that is new philosophically and useful in the advancement of science.

* Received 28. v. 58

¹ P. W. Bridgman, *Reflections of a Physicist*, New York, 1955, p. 25

1 *A Means of Distinguishing between Differences in Meaning*

Here I propose to examine an important aspect of Operationalism occurring very often in Bridgman's analysis yet hardly ever referred to explicitly either by him or by other authors. For the sake of brevity I will call it the 'differential' aspect.

Operational Analysis is commonly understood to consist of an attempt to redefine all our scientific concepts in terms of the operations giving rise to them, rejecting concepts which are undefinable in terms of these operations, as 'meaningless'. After constructing an operational criterion, by deciding on the nature of the operations that will be admissible we are to examine our concepts in the light of it and classify them into 'meaningful' and 'meaningless' ones. A typical resumé along these lines is for instance that of Frank who says:

... a theory which does not contain the operational definitions of its abstract terms is meaningless. In this way he (Bridgman) arrived at a concept of 'meaning' and 'meaninglessness' that was similar to Carnap's.¹

But the above indication of the nature of operational analysis does not take into account the much wider scope of analysis, namely that of revealing the different operational elements amongst the 'meaningful' concepts themselves. For a concept or statement is not just either meaningless or meaningful—according to Bridgman—there are also different sorts of meanings depending on the operations involved. By discovering these differences, we may also anticipate hitherto unsuspected practical differences between the properties associated with various concepts.

Before explaining further the nature and scope of this 'differential' aspect with the help of some practical examples, it will be interesting to read an attack on Bridgman by Margenau which represents a most common criticism on Operationalism and which results from the complete disregard of the differential aspect.

Precisely what is meant by an operation is of concern to those who wish to understand Bridgman's views. Originally there seemed little doubt that experimental operations, processes leading to measurements, were envisaged. Later, however, Bridgman has spoken of paper and pencil operations and even of mental operations. 'This seems like a retreat, for if thoughts were included among the operations, nobody could possibly find fault with operationalism *nor would it be saying much* (italics mine).'²

¹ P. Frank, *Modern Science and its Philosophy*, Cambridge (Mass.), 1950, p. 44

² H. Margenau, *The Nature of Physical Reality*, New York, 1950, p. 232

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Reading Bridgman's analysis will indeed show that he does admit all sorts of operations. Besides the actual instrumental operations, he sometimes allows us to use operations not performable practically but only possible in thought—mental operations. Further, mathematically derived concepts are considered by Bridgman as operationally defined—in a paper and pencil sense. But his efforts are being directed towards detecting the different operational elements that enter into a definition and separating them from each other. For the existence of different operational elements is a most important clue for Bridgman for the existence of difference in meaning which eventually will manifest itself in fundamental practical differences.

A simple example of this 'differential' aspect and how it can lead to practical results is provided by Bridgman when he proposes to resolve the well known 'Gibbs Paradox' in thermodynamics. It illustrates also how misplaced was Margenau's criticism that 'nor would it be saying much':

Given a box with a partition in the centre and on the two sides of the partition two different gases. The partition is removed and the two gases allowed to diffuse into each other. Complete mixing is accompanied by a characteristic and easily calculable increase of entropy, the same no matter what the nature of the two gases. Hence the increase of entropy must remain the same in the limit when the two gases become identical with each other. Here is the paradox, because the parts of a single uniform gas are continually inter-diffusing into each other, and there is no increase of entropy.

To resolve the paradox, we must recognize in the first place that we have here a question of double limits and that the limiting process has been improperly performed. In the original set-up the universe of operations includes the operation by which the gases, originally in the two halves of the box can be distinguished from each other, i.e. the operations by which two 'different' gases can be distinguished. It is assumed that this operation can still be performed and has meaning during all stages of the limiting process, as the two gases are allowed to become more similar. Of course this condition becomes more and more difficult to realize instrumentally, but verbally the formulation remains the same. But in the final step of the limiting process there is an abrupt discontinuity in the instrumental process: the two gases now become 'identical', which means that there is no instrumental operation performable now by which they may be distinguished, but the instrumental operations by which two molecules are to be distinguished would demand a determination of the history of each molecule,

according as it was originally in the right or left hand side of the box. Such operations are only pencil and paper operations, not the laboratory operations on which thermodynamics is predicated and which give entropy its meaning. Since the universe of operations of the limit differs discontinuously from that of the preceding stages, the argument for the continuity of any function such as entropy fails.¹

It is important to follow Bridgman's argument in full detail in order to appreciate the difference between his attempt to solve the paradox and the attempts of others. For others have also based their answer on a discontinuity in the limiting process but their solutions are merely *ad hoc*.

Max Planck² for instance, after showing that the increase of entropy at the diffusion of two gases is given by $(-n_1 \ln c_1 - n_2 \ln c_2)$ —where n_1, n_2 are the number of molecules c_1, c_2 are the concentrations of the two gases respectively—says:

It also appears that the increase of entropy depends wholly on the number of the molecules n_1, n_2 and not on the nature, e.g. molecular weight of the diffusing gases. The increase of the entropy does not depend on whether the gases are chemically alike or not. By making the two gases the same there is evidently no increase of the entropy since no change of the state ensues. It follows that the chemical difference of two gases or in general two substances cannot be represented by a continuous variable. . . .

Thus he states quite clearly that the discontinuity *follows* from the evident absence of increase of entropy and not the opposite way round as it is with Bridgman. For, once we have adopted an 'operational attitude', then without ever having been made aware of the lack of increase of entropy on self-diffusion, thus not being puzzled by any paradoxes at all, the difference in meaning, that might exist between the process of 'diffusion' when applied to different gases and when applied to elements of the same gas, would present itself to our mind. For with a complete absence of convection currents how do we know at all that a continual intermingling of the elements of a gas takes place? This follows from our kinetic picture of gases, according to which a gas, at complete rest macroscopically, consists of molecules rushing about at enormous speeds. But knowledge based on a mental picture is essentially different from that confirmed by instrumental

¹ P. W. Bridgman, *The Nature of Thermodynamics*, Cambridge (Mass.), 1943, pp. 168-9

² M. Planck, *Treatise on Thermodynamics*, Dover Publ., 3rd edn., p. 222

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evidence, whereby we detect intermixing of different gases. Admittedly we would be in no position to foretell the nature of the manifestation—if any—of such difference in the meaning of 'diffusion', but when presented with the paradox all we have to do is to correlate the abrupt change in the behaviour of our system with regard to entropy increase, with the abrupt change in the meaning of the process taking place.

Planck's explanation, however, is an *a posteriori* one. Furthermore his justification of the existence of such discontinuity, namely, that chemical constitution varies in whole numbers,¹ does not satisfactorily explain the special significance of the discontinuity of the last step in the limiting process, because throughout the whole process of approaching similarity we are dealing with a variation in terms of whole numbers.

Or take a different application of the differential aspect of operational analysis of a far wider generality. Even though it is hard to imagine an actual case where the following application can be made practical use of, it is an important indication of how Bridgman's philosophising about science has a bearing on doing science:

Let us say we have established experimentally Law A and derived from it by the logical steps of mathematics Law B. Can Law B now be regarded as having been tested experimentally?

In his discussion Bridgman uses the example of a body falling freely by gravity. Say we have established that $d^2s/dt^2 = g$ by various experiments. Now we integrate twice and obtain $s = \frac{1}{2}gt^2$. Do we have to confirm this result? This is what he says:²

I think a common answer could be that verification is quite superfluous.

... I think a better answer would be that the answer depends on the physical operations which we have chosen to employ to our verification, first of the differential equation and then of the integrated form

... if the set of readings made to verify the differential equation is not the same as made to verify the integrated equation, then verification is not superfluous.

Of course we may object that one can never know with certainty that a differential equation represents a physical situation, as the idea of a limit is a mathematical one. Physically we can only approximate to it. Thus $ds^2/dt^2 = g$ cannot be known with certainty. This difficulty is being noticed by Bridgman:

¹ See also M. Planck, *Treatise on Thermodynamics*, Dover Publ., 3rd edn., p. 24 n.

² P. W. Bridgman, 'The Nature of Some of Our Physical Concepts', this *Journal*, 1951, 2, p. 251.

The last two examples already show that, even conceding to Margenau that by admitting 'thought experiments' Bridgman has left us no non-operational concept at all, this differential aspect of operational analysis has thereby not diminished its significance.

In fact the opposite is true. The scope of the analysis is widened by the admission of thought experiments and fruitful results can be obtained even in mathematics by considering the different operational elements of different mathematical concepts. Bridgman considers only physical examples, but it is easy to think of examples in mathematics. Take for example the concept 'sum'. We know that only after the strange properties of infinite series had kept the seventeenth and eighteenth century mathematicians baffled for a long time, did they start to realise that the 'sum' of an infinite series must be a different concept from the 'sum' of a finite series. It is not really a 'sum' in the same sense but a limiting value.

Now the application of operational analysis could reveal this at once, for the meaning of 'sum' depends on the operation of summing. In the case of finite series summing can always be performed by adding one term to the next, their sum to the next, and so on. With infinite series, however, the process of summing is never actual addition of the individual terms one to the other but some different operation such as the elimination of all the terms except the first and last or integration and the like. Thus the difference in the operation of 'summing' might have served as an indication of the existence of basic differences in the nature of the two concepts.

2 *A Criterion of Physical Reality*

This leads us to an important point in Bridgman's view on the differential power of operations, a point which has again been often ignored by followers and opponents alike. Two different operations only indicate the possibility of differences in the nature of the concepts but they do not necessarily imply such differences. If careful examination shows for example that the concept length, specified by the operation of 'moving a meter stick from one position to the other maintaining itself always parallel to itself', and the concept length 'determined by rotating the stick through 180° for each successive position', do not differ from each other in any way, there is no point in maintaining that we are dealing with two different concepts. Initially we are to be careful and not to take for granted that we are

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just dealing with two different measurements of one and the same concept but say perhaps one measurement represents 'parallel' the other 'rotational' length and expect to find differences between the two just as modern physics did find differences between 'tactual' and 'optical' length, i.e. between distances measured by meter sticks and distances measured by means of Doppler Effect. But when no such differences are revealed then not only should we not insist on the existence of two concepts but on the contrary we should consider ourselves as having proved that we are confronted with a 'real' property, namely length is a real property as opposed to an 'invented' property which is devised for correlational purposes. In fact the main bulk of Bridgman's writing on thermodynamics is devoted to finding out which of the thermodynamical concepts are 'real' and which not. He examines in turn such concepts as 'energy', 'entropy', and the test to be applied is always the same: If we can find two independent operations in terms of which the same concept can be defined then we know that the concept is a 'real' one. For his position seems to be that since properties are best characterised by operations—two operations should be expected to give different results, and if they do not, then by virtue of the differentiating power of operations, we have the strongest evidence that we are dealing with a 'real' property transcending the methods defining it. Thus we are entitled to ascribe physical reality to the concept of 'stress' of elastic bodies, because it is uniquely connected with two independent sets of operations. For one way of defining the state of stress of a body is in terms of the forces acting across its free surface. But as a body under stress is also in a state of strain which may be determined from its external deformations, we are provided with an additional means of defining stress.

It would be interesting to enquire what the 'reality' of a concept or property actually implies, yet without further enquiry it is quite clear from the above that any attack along the lines of Lindsay's attack on Bridgman is unjustified.

Lindsay claimed¹ that the Operationalism of Bridgman implied the tying of a concept to a definite operation. Consequently the temperature of a body measured by Pt.-wire-thermometer means something different from its temperature when it is measured by a Hg-thermometer.

¹ R. B. Lindsay, 'A Critique of Operationalism in Physics', *Philosophy of Science*, 1937, 4

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But such an isolation of concepts could defeat the very aim of physical science, which is to provide a simple and economical description in terms of a minimum number of concepts.

He continues by saying how much effort has been expended to divorce temperature from particular operations:

... culminating in the absolute thermodynamic scale of temperature which is independent of the thermometric substance used. Even more recent is the attempt to define the concept statistically. Now it is hard to believe . . . that these have not deepened our understanding of thermal phenomena. Nevertheless strict adherence to the operational point of view would place them outside the pale.

The above criticism could apply against some of the followers of Bridgman in whose opinions Operationalism does necessarily lead to such extreme attitudes. Thus Dingle, in an article 'A Theory of Measurement',¹ does insist for instance that distances measured directly by laying measuring rods end to end and distances measured by triangulation method where we observe the angles subtended at the eye by the two extremities and then apply calculations are not the same but two distinct quantities which, if we are to be consistent, ought to be called by two different names even, such as, say, 'distance' and 'remoteness'. Or for example he explains that:

... the results yielded by our ten methods of finding surface tensions are not ten strivings after the same ideal but ten independent quantities which we have discovered to be approximately equal.

Not so Bridgman. To him it is only important that we should not take it for granted that the different ways of measuring temperature measure one and the same quantity, but once it has been established that the results of the different measurements differ in no appreciable way one from the other we should expect further manifestations of the very same quantity. Thus the absolute thermodynamical scale of temperature is not outside the plan of operationalism, for Bridgman does not hold that 'temperature is what the thermometer measures' just as he disagreed² that 'intelligence is what the intelligence tests test'.

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¹ H. Dingle, 'A Theory of Measurement', this *Journal*, 1950, I, 5

² P. W. Bridgman, 'Some General Principles of Operational Analysis', *Psychological Review*, 1945, 52

NOTE AND COMMENT

A Theory of Causality

THE view of causality that I shall describe here is a modification of the classical view. The classical view has strong overtones of strict determinism. For example, the throwing of a stone may be regarded as the cause of a window being broken, and the classical analysis would tend to ignore the fact that the window may after all fail to break. If the possibility of this failure to break were taken into account in the analysis it would be ascribed to the fact that other causes were at work. There is, however, a more modern view of causality in which determinism is not taken for granted. The modern or modified view seems to me to be a more useful one than the classical one.

If I speak later about a 'definition' of causality this should not be taken as implying that no other definition is possible, and many readers may prefer to regard the discussion as about a 'theory' of causality instead. But if I am not mistaken the definition corresponds closely to modern English usage. It is just because the definition is mathematical that it cannot be found in any English dictionary. Of course the analysis is not especially tied to the English language.

In a conversation in Chicago in 1955, L. J. Savage remarked that the understanding of probability had reached a point where it should be possible to give a definition of causality. We agreed that (i) the thing to try to do would be to define the expression '*F* is a cause of *E*' (not necessarily *the* cause), where *E* and *F* are events, (ii) the definition should be probabilistic, (iii) if we could prevent *F* we would tend to prevent *E*, (iv) *F* seems to precede *E* in time, (v) it is inadequate to say that *E* and *F* are correlated and that *F* precedes *E* in time, a counter-example being the sensations of lightning and thunder.

I have recently given more thought to the question and have decided, in agreement with Popper, that the definition should be formulated in terms of what he calls 'propensity',¹ i.e. what Ramsey² called a 'chance', has sometimes been called 'physical probability',³ and may also be called 'material probability'. This last expression has the advantage of completing the parallelogram of expressions 'logical implication', 'material implication', and 'logical probability'.⁴

¹ K. R. Popper, *Proc. Ninth Symp. of the Colston Research Soc.*, London, 1957

² F. P. Ramsey, *The Foundations of Mathematics*, London, 1931

³ See, for example, I. J. Good, *Probability and the Philosophy of Science*, London, 1950, p. 116

⁴ In order to prevent misunderstanding I should like to agree with Ramsey that physical probability can be defined in personal

One popular notation for a subjective probability or credibility is $P(G/H)$, read 'the probability of G given H ', where G and H are propositions. When a proposition asserts the occurrence of an event we may use the same symbol to represent both the event and the proposition.

If H_1 represents all the true laws of nature then $P(E/F, H_1)$ can be abbreviated to $P(E/F)$ when we are talking about physical probability. (I am here using the usual logical notation of a period for 'and', and $\bar{E}$ will represent the negation of E .)

Although indeterminism is fashionable people still talk usefully about causality, blame, and responsibility. Campbell¹ pointed out that causality is less important for science than was at one time thought, as for example, by Mill.² Russell³ too says that the idea of causality is useful only 'in the infancy of a science'. But the attempt to define causality is still of interest as an example of mathematical philosophy (rather than of the philosophy of mathematics), and also as a contribution to the definition of the direction of time.

When we try to define the statement 'an event F is a cause of the event E ', it is desirable to avoid reference to time, since Reichenbach⁴ defines the direction of time by assuming that causality is already defined. (That time has a direction means roughly speaking that there is no ordered cycle of events each of which causes the next one in the cycle.)

In the first place we must deny Hume's law that E and F must be contiguous in space and time. Perhaps Hume's law is true for strict causality but the present discussion is concerned primarily with probabilistic causality. For example, a murderer presses a trigger: his victim drops dead. This is cause and effect by common parlance, but the events are not contiguous.

We do not say that an event F caused E unless both events occurred. Accordingly our first pair of conditions is:

C1 and C2. E and F both occurred or will occur, in other words the propositions E and F are both true.

probability. It can also be defined in terms of multisubjective probability, and in terms of credibility (= logical probability = impersonal rational intensity of conviction). Analogously, the physical world can be defined for me, if at all, in terms of my sensations. I have the purely metaphysical belief that the physical world exists and that physical probabilities exist. (I also find it mentally healthy to believe in credibilities and to think of my subjective probabilities as judgments of credibilities.) If this view of physical probability is not accepted the following arguments will not be

¹ N. R. Campbell, *The Elements*, Cambridge, 1919

² John Stuart Mill, *Logic*, Vol. I, 1879, p. 377

³ Bertrand Russell, *Human Nature and Conduct*, 1949, p. 194

⁴ H. Reichenbach, *Experience and Prediction*, Berkeley and Los Angeles, 1956

A THEORY OF CAUSALITY

As before, let H_1 state all the true laws of nature including the ones we do not know. Let H_2 specify the essential physical background or physical circumstances that are liable to be taken for granted, for example, that there was no lead sheet between the murderer and his victim. Let $H = H_1 \cdot H_2$. We shall always regard H as 'given' in the probability notation, so that all our probabilities may be regarded as more or less physical. The next pair of conditions expresses the fact that neither E nor F had to occur.

$$C_3 \text{ and } C_4. \quad P(E/H) < 1, P(F/H) < 1.$$

We may suppose either that H_2 is an incomplete description of the physical circumstances, or we may suppose that it is complete and accept indeterminism. Conditions C_3 and C_4 will then not commit us either to determinism or to indeterminism. 'Pseudo-indeterminism', a form of determinism indistinguishable in principle from indeterminism,¹ will also not be ruled out. This is just as well since the idea of causality should be available to us whatever our metaphysical views may be concerning determinism.

The next condition states in our example that the victim was more likely to die if the trigger was pressed than otherwise!

$$C_5. \quad P(E/F \cdot H) > P(E/\bar{F} \cdot H).$$

If we had $P(E/F \cdot H) = 1$ we would be trying to define old-fashioned (= strict) causality.

The next condition expresses the desideratum that an event is not a cause of itself.

$$C_6. \quad F \cdot H_2 \text{ does not logically imply } E, \text{ although } F \cdot H \text{ might do so.}$$

The six conditions proposed above are by no means enough, not even if we break our resolution and say that F precedes E in time. For F may be the sensation of lightning and E that of thunder. We need a condition that states, roughly speaking, that we could have prevented E by preventing F . So there exists a conceivable event, G , where

$$C_7. \quad G \text{ did not occur}$$

(Rip Kirby failed to arrive in time!), such that G would have prevented F :

$$C_8. \quad P(\bar{F}/G \cdot H) = 1, \text{ or approximately } 1,$$

and such that if there is no interference F did not *have to* happen:

$$C_9. \quad P(F/\bar{G} \cdot H) < 1,$$

and finally such that the question of Rip Kirby's arrival would have been irrelevant if the murderer had not been there:

$$C_{10}. \quad P(E/G \cdot H) = P(E/\bar{F} \cdot G \cdot H) = P(E/\bar{F} \cdot \bar{G} \cdot H) = P(E/\bar{F} \cdot H),$$

or approximately so.

¹ See *Journ. Roy. Statist. Soc., Ser. B*, 20, 1958, 35, 38

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In the thunder-and-lightning example we might try taking G as the tight closing of the eyes. Then the condition $P(E/G.H) = P(E/\bar{F}.H)$ would fail.

The following more complicated example looks at first sight as if it wrecks the definition: Let E = the sensation of thunder at noon on Monday; let F = the sensation of lightning at noon on Monday together with thunder at noon on Tuesday; let G = no sensation of lightning at noon on Tuesday. But in fact G is relevant to E if F is false.

Thus (probabilistic) causality can apparently be defined in terms of physical probability, without reference to time. It is not a tautology that the future cannot affect the present; if it is true it is a law of nature.

Mr L. J. Savage has pointed out that conditions C7-C10 add nothing to C1-C6 since we can take $G = \bar{F}.B$, where B is any event of probability strictly between 0 and 1. I then tried the modification of adding to C8 the condition that $G.H$ must not *logically* imply the falsehood of F (the equations in C10 then being only approximate). But then Mr E. M. L. Beale showed that C7-C10 still appear to be vacuous. The reason is that, given C1-C6, there exists G that prevents F and also reduces the probability of E to make $P(E/G.H) = P(E/\bar{F}.\bar{G}.H)$. This equation appears as a 'numerical coincidence', so that G is artificial.

What I had in mind was that G should provide a 'natural' method of preventing F , but it is difficult to give a formal definition of 'natural'. Perhaps we should say, not merely that G exists, but the conditions must also be satisfied whenever the parameters in G are varied slightly. It seems possible that there is an essential vagueness in the notion of causality. At any rate the present note may help to pinpoint the difficulties.

E may be a cause of F since G , F , E may be in reverse time order, but the laws of nature are different when time runs backwards. Apparently causality and time-direction should be axiomatised jointly.

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DISCUSSIONS

CORROBORATION VERSUS INDUCTION

In a recent note 'The Paradoxes of Confirmation' (this *Journal*, 1958, 35, 227-233) H. G. Alexander discusses the problem of what evidence constitutes an empirical support to a hypothesis. His two main points are these. He criticises J. W. N. Watkins' view on the matter; and presents an alternative view. Further, he also claims that Popper's theory of testing is yet another form of induction. Before proceeding to refute each of these points, I should like to say this. Alexander seems to base his criticism of Popper's theory solely on the brief remarks of Watkins. Had he taken account of Popper's own papers, at least of the one in this *Journal* to which Watkins refers, I do not believe that he could have arrived at his conclusions, and the present unfortunately lengthy comments would have been quite redundant.

I

It may be tempting to accept intuitively the following two propositions:

- (a) The hypothesis 'All ravens are black', for example, is empirically backed by observing a black raven but not by observing a white shoe.
- (b) Any hypothesis has the same empirical status however we formulate it verbally.

I shall take (b) for granted throughout the present paper and discuss in the present section the reasons for accepting or rejecting (a).

It seems (as Nicod suggested), that *an observation backs a theory if and only if it satisfies it or is an instance of it*, in the sense that a black raven is an instance of 'All ravens are black'. This suggestion of Nicod seems to explain or justify (a), for we tend to regard a black raven but not a white shoe as satisfying, or as an instance of, our hypothesis. Yet this is a mistake, as Hempel showed. 'All ravens are black' can be reformulated as 'All non-black things are non-ravens' and then it will be satisfied or instantiated by any object which is a non-black non-raven—for instance, a white shoe. Hempel concludes that on Nicod's suggestion *every object which does not refute a hypothesis supports it*.

This conclusion of Hempel's is known as Hempel's paradox of confirmation, or simply *the paradox of confirmation*. The word 'paradox' is used here not in the sense of 'logical paradox' but in an everyday sense, so that it is not obvious what a philosopher intends to avoid when he says he intends to avoid the paradox of confirmation. He may intend to deny Hempel's conclusion from Nicod's suggestion, or he may try to avoid the inconsistency involved in the acceptance of both (a) and Nicod's suggestions. It is clear from Hempel's argument that we have to reject the conjunction of (a) and Nicod's suggestion; we may, however, choose one of the following three alternatives. First, we may accept Nicod's suggestion while

rejecting (a) itself. Secondly, accepting (a) we may reject Nicod's suggestion as an unsuccessful attempt to explain or justify it. Thirdly, we may reject both (a) and Nicod's suggestion.

Hempel accepts the first alternative: he accepts Nicod's suggestion and rejects (a). Watkins rejects the first alternative, for reasons to be discussed in the following sections. He does not state explicitly whether he accepts the second or the third alternative, namely whether he accepts (a) or rejects it as well.

Now Alexander shows that (a) is in conflict, not only with Nicod's suggestion (which Watkins rejects), but also with Popper's suggestion (which Watkins offers in its stead).

Popper suggested that *a piece of evidence backs a hypothesis only if it was obtained through an unsuccessful attempt to refute it*. Now it is quite conceivable that we shall see a white object which looks like a raven, and, rushing to examine it in order to refute 'All ravens are black', find that it is not a white raven but, say, a white shoe (or, better, a white bird which looks not unlike a raven). We may examine a cupboard having good reasons to suspect that it contains a non-black raven and find that it contains only a white shoe. In these cases we shall, according to Popper's suggestion, accept the evidence as backing our hypothesis, quite in opposition to (a). Alexander concludes that by Popper's suggestion *every piece of evidence which does not refute a hypothesis may back it*.

So far I fully accept Alexander's argument. I now wish to discuss a further conclusion which he draws from the above, namely 'that Watkins does not succeed in solving or dissolving the paradoxes' (p. 229). I find it somewhat surprising that having succeeded in drawing from Popper's suggestion the conclusion that any object *may* provide supporting evidence non-refuting, Alexander finds it sufficient to declare this conclusion to be an argument against Popper's suggestion and thus close the critical part of his paper with no further explanation.

It seems to me that Alexander *either* (i) claims that Watkins is inconsistent in accepting Popper's suggestion (only unsuccessful attempts to refute a hypothesis support it) while rejecting Hempel's conclusion (all non-refuting objects support a hypothesis); *or* (ii) that Watkins is inconsistent in accepting Popper's suggestion while accepting (a); *or* both. Since Alexander rejects Popper's suggestion with no further explanation, and since he criticises Watkins for supposedly accepting (a) (p. 233, top of page), he seems to be launching both criticisms at one and the same time. But in both cases he is wrong.

As to the alternative (i): Hempel's conclusion from Nicod's suggestion is: every object which does not refute a hypothesis *supports* it. Alexander's conclusion from Popper's suggestion is: every object which does not refute a hypothesis *may* support it. The two are by no means identical and we may reject the one and admit the other.

As to alternative (ii): Although Watkins did not state explicitly that he rejects (a), he did reject it by clear implication. In the passage which serves as Alexander's main text (p. 228), Watkins says (*Philosophy*, 1958, 35, 351) that 'on a Popperian theory . . ., statements about non-ravens which do not report tests of our hypothesis ['All ravens are black'] cannot confirm it' (i.e. support it). Referring to this very statement Alexander claims (p. 233) that Watkins 'says that statements about non-ravens cannot confirm it [i.e. support the hypothesis 'All ravens are black'] because

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they do not report tests of the hypothesis'. I find it strange that Alexander mis-states the main text on which he bases his criticism, especially in view of the following two facts. First, he labels his mis-statement as 'Watkins' position as actually stated by him' (p. 233 first line). Moreover, Watkins' statement occurs in a bracketted passage (Alexander omits the brackets) which refers to a previous paper in which Watkins gives his explanation of why (a) sounds plausible, just as Alexander gives (p.232) his explanation of why (a) sounds plausible. (Both of them refrain from an explicit rejection of (a).)

Watkins' explanation of why (a) sounds plausible (*Philosophy*, 1957, 33, 118) is this.

Assume that in an attempt to refute 'All ravens are black' we ask a zoo-keeper whether he has ever seen or heard of a non-black raven, assuming such a person to be the most likely to know of such a bird if it exists. His answer, if it amounts to the assertion that he had never seen a non-black raven, will normally be something like this: 'No; all ravens I have ever seen or heard of are black.' This makes (a) sound plausible. Yet, Watkins adds, if we merely ask the man to tell us about the black ravens he has seen or heard of, his report 'I've seen hundreds of thousands of black ravens' will *not* count as backing the hypothesis. (Thus, again, by implication Watkins definitely rejects (a).)

My own position is even more radical than Watkins'. I would reject (a) even if it were perfectly acceptable on intuitive grounds, because I do not accept intuition as the high court of appeal. If I were to defend my rejection of (a) on intuitive grounds I should do it in roughly the following way.

Consider Galileo's hypothesis: it is the assertion that all freely falling bodies move with a constant acceleration. Had we accepted Hempel's view, we would consider the behaviour of all non-freely falling bodies—say, the bow in the hand of the violinist—to provide empirical backing to Galileo's hypothesis. This is evidently unacceptable. On the basis of (a) we would just repeat one experiment in order to back the hypothesis. In fact, more and more ingenious ways of testing it were invented. The most beautiful example is, perhaps, Newton's experiment in which a feather and a lead ball fall almost in unison in a vacuum tube which was quickly turned upside down. Another test, which refutes Galileo's hypothesis, is to compare the acceleration of falling bodies high above the surface of the earth and low in deep mines. This experiment was suggested already by Bacon, though it could not be performed then (for lack of precision instruments). Long before this experiment was performed it was clear from general considerations and from Newton's theory, that Galileo's hypothesis is false.

That much against that part of (a) which says what evidence is relevant. As to the part of (a) which says what evidence is not relevant, consider the following possible test.

Galileo's hypothesis can be tested by dropping steel balls off an electromagnet by switching off the electric current. These experiments can be carried out in deep mines to allow the steel balls to fall freely a long way. Now suppose that for a segment of its path a ball moved with variable acceleration. This would count as a refutation of the hypothesis, unless we deny that the fall was free. Suppose someone denies this. If he can show, say, that the mine contained a magnetic rock which altered the acceleration, we may surely consider this whole experiment as empirical support for Galileo's hypothesis. The support will be stronger if before carrying out the experiment we

had tried to make sure, using all our knowledge of the earth's magnetism, that no magnetic rock exists in the mine.

We get a similar situation if we substitute Newton's hypothesis for Galileo's and optics for magnetism. Bradley's discovery that the apparent displacement of planets is due, not to their disobeying Newton's laws, but to the aberration of light, was considered as support for Newton's theory. Similarly, Laplace's discovery that the alleged irregularities of planetary motion (i.e. their alleged violations of Newton's laws) were merely the results of inaccurate calculations, was considered as support for Newton.

These test-situations are quite analogous to these described by Alexander where a suspected non-black raven turned out, on closer examination, to be a non-raven after all. This, of course, does not imply that *all* non-ravens provide support for 'All ravens are black'—only non-ravens involved in *testing this* hypothesis may be of empirical significance for it.

2

I now come to Alexander's two alternative suggestions (p. 232) as to what empirical evidence may count as backing for a hypothesis. (I should add at once, perhaps, that I accept both his suggestions as necessary—they follow from Popper's suggestion—but not sufficient.) The first is that the better the evidence the more improbable it must be. The second is that the evidence has to be improbable not given that the hypothesis is true, and probable given that the hypothesis is true; the greater the difference between the probability of the evidence given the hypothesis, and its probability not given the hypothesis, the more the evidence backs that hypothesis.

Alexander says (p. 232), that 'the probability of something being a raven is less than the probability of its being non-black' then 'this is a black raven' will, as the less probable evidence, support 'All ravens are black' more than 'This is a non-black non-raven'. Thus, Alexander claims, on either of his suggestions, he escapes Hempel's conclusion. But I must confess that I fail to see his point. The evidence in question is not 'This is a non-black non-raven', but rather 'This is a white shoe'—as highly improbable a piece of evidence as 'This is a black raven'!

Let us repeat Alexander's example by which he repudiates Popper's suggestion. Assume that we look for improbable evidence, and that we know it to be very (though not entirely) improbable that we shall find anything at all in the cupboard. Searching it for black ravens we find a white shoe in it. Since it was very probable that we should find nothing there, it was improbable that we should find a white shoe there. Thus, by Alexander's first suggestion the evidence should count as backing the hypothesis. Similarly, by his second suggestion the evidence again should count, since the same improbable white shoe becomes more probable on the basis of any hypothesis which excludes some possibilities. Thus, by his own criticism of Popper's view his two suggestions have to go. Moreover, his two suggestions, far from explaining (a), actually contradict it.

Watkins rejects Hempel's view on the following grounds. However you limit the field of acceptable evidence, within this field there will exist evidence not conflicting with our hypothesis, and perhaps also evidence which does conflict with it.

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Unless we demand the search for refuting evidence, we are in the field of acceptable evidence, only for that evidence which confirms our hypothesis, and be blind to the refuting evidence. This tendency to find empirical backing to a false hypothesis is to limit our evidence to that gained by attempts to refute it. (a) is often acceptable would make it possible to back such a hypothesis like faith healing, by marshalling evidence in accordance with the hope that the evidence must be improbable will not help, since it is as improbable as any. Even his second suggestion will not be as empirically backed. Pseudo-science will thus become science. Alexander's suggestion. Moreover, they will turn scientific science. For, as Galileo's theory was once backed by experiments, we have only to repeat these experiments (with irrelevant variations) to increase the likelihood of it being refuted. On Popper's view such attempts in the hope of supporting the theory, would have no place. Any attempts to back it will have to consist of sincere attempts to refute it.

To conclude, I think that Alexander's view is hit by the criticism he tries to launch against Watkins. Moreover, as Popper has said in 'Notes on Confirmation or Corroboration' in this *Journal* (1958, 149 n.), Alexander's conditions for the acceptability of a hypothesis follow from Popper's conditions. Had Alexander taken account of Popper's three papers he could not have failed to notice that his view is equally applicable to his own and to Popper's suggestion. The probability of a black raven to be in the cupboard is the same as estimating the probability of a black raven to be there to be rather high. He might also have mentioned the attributes mentioned in the hypothesis but rather the hypothesis is improbable.

3

Alexander interprets (p. 229) Popper's theory of corroboration as empirical support, as higher level induction: hypotheses which raise the probability of similar failures in future observations. This misinterpretation is only too common; quite a few people have misunderstood Popper's intention was to justify the authority of empiricism by substituting observations of tests for observations of instances. Popper has to him the view that *science has authority*, that we are not to accept scientific hypotheses whether we like it or not. I therefore present arguments both against the view that empirically backed hypotheses have authority, and against the view that the aim of science is to test hypotheses by entrenching them, rather than with the hope of refuting them.

Science, or the scientific tradition allows a young scientist to deny the truth of the most authoritative and well-established theory, and to offer an incredible alternative to it. I have heard of a young and unknown Einstein. Some people are wise after the event (especially since Whittaker's attack on Einstein) that by the time Einstein had lost its authority, or that Einstein's theory does not exist.

these claims are untrue. What Michelson's experiment refuted was not Newton's theory but the conjunction of it and Maxwell's theory. Kelvin claimed in 1904 that it must be possible to explain Michelson's experiment through modifying scientific ideas other than Newton's theory (preface to second edition of his *Baltimore Lectures*), and his view was the majority view (in fact the view accepted by all save Poincaré, who had a *programme* for an alternative theory but not an alternative *theory*). The history of the lively discussion between the Newtonians and the Einsteinians is not so old as to be entirely forgotten.

Quite generally, any attempt to attribute authority to existing views will be a discouragement to rebels who may have radically new ideas. The growth of science is the abandonment of temporarily 'established' views. Therefore we must encourage rebels, even before their success, rather than discourage them by insisting on the authority of the accepted views.

Those who deny that new scientific ideas conflict with old ones may still admit that the aim of science includes the elimination of errors and the discovery of new facts. Yet the only way to eliminate errors is by criticism, including experimental refutations. And the only way to strive for new discoveries is to attempt to refute accepted views, as I shall try to illustrate by an example.

People seldom ask how discoveries are made. I have found no discussion or explanation of how Gilbert discovered the inclination of the compass. A moment's thought will reveal that the crude instruments of his time could not possibly allow such a delicate variation to be observed without any special preparation. How did Gilbert come to make the necessary preparation? I mention this very example here because I have discussed the revolutionary philosophy of Gilbert somewhere else, alluding to this experiment (this *Journal*, 1958, 35, 238). It may be useful to indicate how the discovery was made as a refutation of an (accepted) hypothesis. The hypothesis (of Paracelsus and Carden) was that the compass is attracted by the Pole Star. As Gilbert tells us he started his magnetic investigations in an attempt to test it. His reason, I conjecture, was that if Copernicus' hypothesis is true, then the pointing at the Pole Star of a freely suspended compass should reflect the annual rotation of the earth. (This is, of course, a rather naïve estimate of the distance of the fixed stars from the solar system.) The inclination was found, but it was not towards the Pole Star. Explaining it as pointing towards the north pole Gilbert conjectured that the earth is a huge homogeneous magnet. Tests of this hypothesis of Gilbert showed the existence of unexpected variations of the earth's magnetism; thus the field of research on the earth's magnetism was opened by the refutation of Gilbert's hypothesis.

This example is an attempt to illustrate how the discovery of new facts takes the form of refutations of older hypotheses (in accordance with Popper's theory). I think that my interest in correcting our errors (i.e. trying to refute them), in finding new ideas (which clash with old ones), or in finding new facts (which are refutations), must clash with the vulgar view that empirically backed hypotheses have any authority whatever. Science does not oppose freedom of thought but rather depends on it.

One question remains. If we are interested in empirical refutations, why have a theory of empirical support? In other words, what is the function of Popper's theory of corroboration?

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There are a number of answers to this. The first is this. Popper argued that the empirical backing of which people speak is not the evidence which increases the probability of a hypothesis but that which was obtained through sincere attempts to refute it. The second is this. The more corroborated a hypothesis is the more important is the discovery which refutes it (Michelson's experiment, Hertz's photo-electric effect, etc.). The third is this. Any well corroborated theory (say Maxwell's) must be a first approximation to the theory which supersedes it: the new theory has to explain how the old theory, though false, stood up so well to severe tests (like Hertz's).

To conclude: Popper's theory of corroboration is not hit by the paradox of confirmation, but dissolves it by advancing a view of scientific thinking and research as unauthoritarian, imaginative, questioning, and rebellious; a view according to which neither black ravens nor non-black ravens nor even Michelson's experiment confirm, or sanctify, or give authority to a hypothesis. Popper's philosophy is a philosophy of criticism. Corroboration of a theory is merely an appraisal of the way it stood up *so far* to severe criticism. This appraisal may help those who try to criticise a theory again, for they may wish to know what happened to previous attempts. But even a high degree of corroboration should not discourage the critic. It may even be a challenge.

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LATTICE STRUCTURE OF SPACE-TIME

ON pages 106-107 of Abramenko's article, on the dimensionality and continuity of space and time,¹ he produces an argument that is intended to support the theory that space-time has a lattice structure. Essentially this is the 'chronon' theory mentioned, for example, by Whittaker,² who says:

In fact, some physicists in 1928 made the suggestion (which, however, was not favourably received) that this might actually be the case, and they proposed the name *chronon* for a time-unit of about 4.5×10^{-24} sec., which would correspond so to speak, to a single picture in the cinema of existence.

Clearly the chronon is $h/(m_p c^2)$, where m_p is the proton mass, and the corresponding smallest space unit is $h/(m_p c) = \lambda_{cp}$, the Compton wave-length of the proton.

For convenience I quote Abramenko's argument:

Let us consider a physical point-instant, or quantum of space-time, as an irreducible spatio-temporal volume defined by the expression:

$$V_{tr} = \frac{4}{3}\pi l_0^3 \tau_0.$$

Assuming l_0 to be of the order of magnitude of a proton radius, i.e. $\sim 1.5 \times 10^{-13}$ cm., and the value of $\tau_0 = l_0/c = 0.5 \times 10^{-22}$ sec., we obtain the value of $V_{tr} = 7.1 \times 10^{-62}$ cm.³ sec. If we imagine this spatio-temporal quantum to be filled with matter of nuclear density, $\sigma \simeq 10^{14}$ g/cm.³, its energy content will be

$$V_{tr} \sigma c^2 \simeq 6.4 \times 10^{-27} \text{ erg-sec.},$$

which is of the same order of magnitude as Planck's quantum . . .

¹ B. Abramenko, 'On Dimensionality and Continuity of Physical Space and Time', this *Journal*, 1958, 9, 89-109

² E. T. Whittaker, *From Euclid to Eddington*, Cambridge, 1949, p. 41

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By finishing up with an apparent numerical coincidence this argument is made to appear much stronger than it really is. The Compton wave-length of the proton may be taken as one definition of the radius of the proton. Then the nuclear density is $\sigma = m_p / (\frac{4}{3}\pi\lambda_p^3)$. It is now a question of algebra (not arithmetic) to see that $V_{tr}\sigma c^2 = h$. Thus the apparent numerical coincidence is a tautological deduction from simple known atomic theory. It would have been better to present the argument thus:

The energy in a proton ($m_p c^2$) multiplied by the 'chronon' ($h/(m_p c^2)$ sec.) is equal to h .

Put this way the argument may be worth something but not very much. Personally I quite like the lattice theory, but this is rather beside the point.

On pages 107-108, Abramenko leaves an argument less complete than necessary. If the total number of spatio-temporal quanta is 10^{108} , and if we assume that in each of them an elementary particle is created whose mass has probability $\frac{1}{2}$ of being positive and probability $\frac{1}{2}$ of being negative, then after these have paired off with destruction, the number of particles left will be of the order of 10^{81} . It is not necessary to assume that time is curved on itself, only that the process has been going on for a few thousand million years. For example, if space is infinite and the universe is expanding then the argument may apply to the observable universe. The question arises in this case how large each positive and negative concentration of matter may be expected to be. As a statistical problem it is rather like the problem of the distribution of sizes of connected sets ('armies') in the game of Wei Chi (or Go) the Japanese national pastime), in which black and white pieces are placed on a two-dimensional lattice.

The weakness of the theory is that it says nothing about how far apart in space and time mutually annihilating pairs must be. Given a more complete formulation it may be possible to obtain corroboration or disproof. The theory should tell us, for example, something about the best possible vacuums. A further question is what happens if a particle tries to come into the world at a position that is already occupied?

Regarding the question of the dimensionality of space, I find that I can place six rods of equal length to form a regular body (a tetrahedron), but I cannot add another four rods (of the same length) as I should be able to do in four spatial dimensions. This seems to show that space is locally three-dimensional, even if its structure is microscopically granular or macroscopically curved or even multiply connected.

In an earlier paper on the dimensionality of space, Whitrow¹ makes the interesting remark that the orbit of the earth would be unstable if the number, n , of dimensions of space, exceeded 3, and that therefore $n = 3$ is a necessary condition for the existence of the formulators of the question of why $n = 3$. Likewise Courant² has stated that only in three dimensions is high-fidelity signal transmission possible. Hence Whitrow's remark may be generalised to any communication systems, not just to human nervous systems.

¹ G. T. Whitrow, 'Why Physical Space has Three Dimensions', this *Journal*, 1955, 6, 13

² R. Courant in *Modern Mathematics for the Engineer*, ed. by E. F. Beckenbach, New York, 1956, p. 101

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The following suggestions all seem to be worth some consideration.

(i). God selected $n = 3$ in order to make our existence, and that of other communication systems, possible; or

(ii) $n = 3$ just happens to be the case, with no explanation possible. It is a small integer and not very surprising *a priori*. If the number of dimensions had been say 386491 then we should have considered an explanation to be far more pressing; or

(iii) *we happen to be the right size* to think that $n = 3$. We do not have enough evidence to be sure how many dimensions space would seem to have if our lengths were of the order of magnitude of 10^{-24} cm., or 10^{24} cm., or 10^{48} cm. Perhaps for any given n there could exist beings of a size for which n would seem to be the most natural dimensionality, for events on a scale comparable with their own size. An analogy could be drawn with people living in greatly separated houses, numbered from 1 to 100. Someone living in a house number 17, and unaware of the existence of the other houses, might ask 'Why should a house have the number 17 on it?' (Perhaps I should mention in passing that even fractional dimensions have been suggested by Ulam¹ to have physical significance); or

(iv) even more speculatively, there may be more than three dimensions of space, perhaps even an infinite number, but we can fully appreciate only three dimensions because we ignore low-fidelity signals. I do not see how to formulate this last suggestion even reasonably rigorously, but it seems worth listing here in view of Courant's statement mentioned above.

L. J. GOOD

'THE TWO THESES OF METHODOLOGICAL INDIVIDUALISM'

HAVING replied at some length² to an earlier criticism by Mr Leon J. Goldstein³ of methodological individualism in social science as I originally put it forward in this *Journal*,⁴ I shall only make one small historical comment, in an endeavour to keep the record straight, on his latest attempt⁵ to cast doubt on this excellent principle.

He announces, with the air of making a discovery, that 'the fact of the matter is, however, that under the rubric "methodological individualism" Watkins subsumed two positions'; the first of these positions, Goldstein says, is not methodological at all but 'ontological', while the other is 'more truly methodological' (pp. 2-3). Later he adds that methodological individualists will never be able to come to grips with their opponents' case so long as they 'do not trouble to distinguish between the two kinds of individualism they support' (pp. 3-4).

¹ S. Ulam in *Applied Probability*, ed. by L. A. MacColl, New York and London, 1957

² J. W. N. Watkins, 'The Alleged Inadequacy of Methodological Individualism', *The Journal of Philosophy*, 24th April, 1958, 55, 9

³ L. J. Goldstein, 'The Inadequacy of the Principle of Methodological Individualism', *The Journal of Philosophy*, 6th December, 1956, 53, 25

⁴ J. W. N. Watkins, 'Ideal Types and Historical Explanation', this *Journal*, 1952, 3, 22

⁵ L. J. Goldstein, 'The Two Theses of Methodological Individualism', this *Journal*, 1958, 9, 33

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I made this obvious distinction long ago in the paper which Goldstein originally attacked.¹ Briefly, I subscribe to a metaphysical statement and a methodological prescription, as follows:

(1) Human beings (together with their material resources and environment) are the only causal factors in history.

(2) Explain all social events in terms of human factors.

I gather that Mr Goldstein also subscribes to (1) but wishes to replace (2) by:

(2') Explain some social events in terms of non-human factors.

The burden of his latest paper is that one can subscribe to (2') without abandoning (1). But if one accepts that 'Ought' implies 'Can' acceptance of (2') will involve abandoning (1), since (2') prescribes what (1) declares to be impossible.

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¹ 'The principle whose status I have been trying to elucidate is a methodological rule which presupposes the factual assertion that human social systems are not organisms in the above sense.' 'The ontological basis of methodological individualism is the assumption that society is not some unimagined sort of organism, but really consists only of people . . .' ('Ideal Types and Historical Explanation', *Readings in the Philosophy of Science*, ed. Feigl and Brodbeck, pp. 731, 732.)

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Undecidable Theories. By Alfred Tarski in collaboration with Andrzej Mostowski and Raphael M. Robinson.

North-Holland Publishing Co., Amsterdam, 1953. Pp. xxi + 98. 18s.

THIS monograph, divided into three papers I-III, originated from several combined investigations first by Tarski and Mostowski, later by Tarski and R. M. Robinson, with contributions also by Julia Robinson. Much of the results has been presented already at the meeting of the Association for Symbolic Logic in Columbus, Ohio (December, 1948). Abstracts of these reports were published in the *Journal of Symbolic Logic*, 14 (1949).

The monograph gives not only a detailed explanation of the subject together with the proofs involved, but also a certain widening of the concepts due to a slight modification of the general schema of metatheoretical investigation. Usually formal calculus and its interpretation, and correspondingly syntax and semantics are distinguished by a twofold rôle attributed to the logical laws as rules or initial formulae for the calculus, and also as rules of truth to which the definition of truth has to be adapted.

Now the two concepts of formal derivability and of truth are regarded by Tarski as special cases of a concept of 'validity' which he takes as constitutive in the characterisation of a 'theory'. The theories which are treated in the monograph are supposed to be in *standard formalisation*, i.e. formalised in the frame of the first order logical calculus, with equality and its axioms included. Such a theory is understood to be determined by its (finitely or denumerably many) constants and by the set of its valid sentences (i.e., valid closed formulas). The constants are (a) the logical constants, among which also the quality sign counts; (b) non-logical constants, which are individual-symbols or predicates or functors.

The set of the valid sentences is assumed to include all logical identities (as also those of equality) and to be closed with respect to logical derivation.

In the case where the valid sentences of a theory can be characterised as those which are derivable from a recursive set of 'axioms', the theory is said to be 'axiomatisable' and in particular 'finitely axiomatisable' if the number of axioms is finite.

But the valid sentences can also be determined by an interpretation of the non-logical constants with reference to a domain of individuals; such an interpretation combined with the usual interpretation of the logical symbols gives every sentence formed with the constants a truth value; and as valid sentences are defined those with the value 'true'. More generally validity can be defined by means of a *class* of interpretations of the non-logical

symbols in the way that a sentence is defined as valid if it holds true for every interpretation belonging to that class. For any such definition of validity the set of valid sentences of a theory is closed with respect to semantical inference, so that the semantical consequences of valid sentences are again valid.

It is to be noticed that we need not distinguish here between semantical consequence and derivability, since we have to deal always with standard formalization to which Gödel's completeness theorem applies.

The theorems proved in the monograph are about undecidability of theories. A theory is called decidable if there is for its sentences a procedure of deciding on their validity, or what amounts to the same, if with respect to a proper Gödel-numbering of the set of valid sentences: the set of their Gödel numbers is (general) recursive.

As trivially decidable theories we have the inconsistent theories wherein some sentence, together with its negation, is valid and hence by the closure property all sentences are valid. A consistent theory is called complete if for each of its sentences A either A or $\sim A$ is valid. If a complete theory is axiomatisable, it is also decidable, as follows by an argument of Kleene. Generally a decidable theory is also axiomatisable.

Among the undecidable theories Tarski distinguishes as 'essentially undecidable' those which are not *sub-theories* of decidable consistent theories with the same constants. Here a theory T_1 is called a sub-theory of a theory T_2 if every valid sentence in T_1 is also valid in T_2 , so that the set of valid sentences of T_2 is an extension of the corresponding set of T_1 . Tarski therefore briefly calls T_2 an extension of T_1 , when T_1 is a sub-theory of T_2 . We have, however, to be aware that an extension in this sense does, in general, not amount to a widening of a theory, but often rather to a restriction. This especially appears by the various instances which occur of theories which are undecidable though not essentially undecidable. A pregnant instance of this kind, to which Tarski points, is that of a theory with only one extralogical symbol: a binary predicator P , whose only valid sentences are the logical identities. This theory, by Church's theorem of the unsolvability of the Entscheidungsproblem for the first order logic together with Kalmár's reduction of the Entscheidungsproblem to formulas with only one binary predicator, proves to be undecidable. But in many ways we can get from it a decidable theory simply by adding a non-logical axiom, for instance $(x)(y)(x = y)$ or $(x)(y)P(x, y)$ or $(x)(y)(P(x, y) \longleftrightarrow x = y)$.

Concerning the ways of proving undecidability Tarski distinguishes a direct and an indirect method.

The application of the direct method is concentrated in one theorem (II, Th.1) with corollary, which, by the union of the syntactic and the semantic aspect, generalises a theorem of Rosser on undecidability of formal systems and at the same time Tarski's former statement of the indefinability of truth in derivative systems. The said theorem states that if T is a consistent

theory with terms $\Delta_0, \Delta_1, \Delta_2, \dots$ containing no variables and for the formulas of T a Gödel-numbering is fixed and a variable u is distinguished, if further $D(k)$ is the arithmetic function which assigns to the number k the Gödel-number of the formula to be obtained from the formula with the Gödel-number k by substituting Δ_k for the variable u , and if $D(k)$ is definable in T , then the set of Gödel-numbers of valid sentences of T is not definable in T . Definability of a set s means here that there is a formula $C(a)$ in T , with the only free variable a , such that for every natural number k the sentence $C(\Delta_k)$ or its negation is valid in T according as k belongs or not belongs to s . In a corresponding sense 'definability of a relation in T ' is understood. And definability of an arithmetic function $\phi(k_1, \dots, k_r)$ in T means that there is a formula $\Phi(a_1, \dots, a_r, b)$ in T , with only the indicated free variables, such that for any natural numbers $k_1, \dots, k_r$ the sentence $(x)(\Phi(\Delta_{k_1}, \dots, \Delta_{k_r}, x) \leftrightarrow x = \Delta_{\phi(k_1, \dots, k_r)})$ is valid in T . (By admitting descriptions we could express the last condition on Φ by requiring the equation $\Delta_{\phi(k_1, \dots, k_r)} = \iota_x \Phi(\Delta_{k_1}, \dots, \Delta_{k_r}, x)$ to be valid for any natural numbers $k_1, \dots, k_r$.)

The proof of the theorem is by means of Gödel's method of sharpening the antimony of the liar.

Now this theorem is applied to the formalised arithmetic of natural numbers N and some subtheories of it. Here the non-logical constants are the individual symbol O , the unary functor S and the binary functors $+$, $\times$. The valid formulas of N are those which hold with respect to the domain of natural numbers for the interpretation of O as null, of S as the successor and of $+$ and $\times$ as sum and product.

The theory N , determined in this way is complete. As Gödel has shown, every primitive recursive function is definable in N , and therefore also every class $\{x | P(x)\}$ where $P(x)$ is expressible by a recursive relation and quantifiers, in particular every recursively enumerable class.

Since we have also in N the sequence of terms $O, SO, SSO, \dots$ which figure as numerals and the function $D(k)$ is (by the general properties of Gödel-numbering) primitive recursive and thus definable in N , all premises of theorem II.1 are satisfied for N . Hence by this theorem the set of valid sentences (i.e. of their Gödel-numbers) cannot be definable in N , therefore it is not recursive and even not recursively enumerable, so that N is neither decidable nor axiomatisable. This result can as well be drawn from Gödel's general theorem on formal undecidability by which an axiomatisable theory wherein every primitive recursive function is definable cannot be complete. But theorem II.1 can also be applied to sub-theories of N which are axiomatisable. For this application it is sufficient to show that in the considered sub-theory every primitive recursive function (and thus in particular $D(k)$) is definable. From this it then follows that the sub-theory in question is undecidable, and even essentially undecidable.

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A theory of this kind is Peano's arithmetic, whose non-logical axioms are: $(x)(y)(Sx = Sy \rightarrow x = y)$, $(y)(0 \neq Sy)$, the recursive equations for sum and product, but with general quantification applied to the variables; and the infinitely many sentences which are instances of the formula schema of induction.

In fact this theory P (which is equivalent with the system Z in *Grundl. d. Math.*) is known to have the property that every primitive recursive function is definable in it, and so it follows that this axiomatisable theory is essentially undecidable. In the present monograph the same is shown for that finitely axiomatisable sub-theory of P , called Q , which results from P by cancelling from the list of axioms the instances of the schema of induction and adding instead only the sentence $(x)(x \neq 0 \rightarrow (Ey)(x = Sy))$. This system Q was substituted by R. M. Robinson as a simplification to another system chosen for the same purpose by Mostowski and Tarski.

For the proof that in Q every recursive function is definable a further theory R is introduced which is a sub-theory of Q , but with infinitely many non-logical axioms: Even in this very weak theory, as is shown, every recursive function is definable and so also this theory R is found to be essentially undecidable.

Further to theory Q , the proof is given that *none* of the sub-theories arising from it by cancelling one of the non-logical axioms is still essentially undecidable.

The essential undecidability of Q results by the direct method. However this theory for itself has only a minor interest, as it is only a fragment of number theory. The consideration of it serves mainly for the passage to other theories and thus for the use of the indirect method of proving undecidability. This method consists in the application of certain theorems allowing inference from the properties of one theory to corresponding properties of another theory.

A series of theorems of this kind is proved in the first part of the monograph. In particular they concern the consequences of the interpretability of one theory in another. A theory T_1 is said to be interpretable in T_2 if every valid sentence of T_1 is derivable in a theory T_1^* arising from T_2 by adding to it the non-logical constants of T_1 which are not in T_2 and also axioms which are definitions of those constants, *i.e.* explicit definitions in the case of the predicates to be defined, and in the case of functors or individual symbols to be defined, such formulae as one would obtain from explicit definitions formed with the help of descriptions (introduced by the ϵ -rule) by the subsequent elimination of the ϵ -symbols.

A theory T_1 is called 'weakly interpretable' in T_2 , if T_2 is a sub-theory of a consistent theory T_2^* with the same constants as T_2 , such that T_1 is interpretable in T_2^* .

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Sometimes the interpretation of a theory T in another one requires a preparation, namely when the individuals of T have to be identified with only those individuals of the second theory which satisfy a certain condition. In order that this can be done by an explicit definition, we have first to make the domain of individuals of the one theory formally explicit.

This procedure consists in the following : a unary predicator P is added to the constants of T ; with every sentence Φ of T a sentence $\Phi^{(P)}$ is correlated wherein each constituent $(x)\Psi(x)$ of Φ is replaced by $(x)(P(x) \rightarrow \Psi(x))$ and each constituent $(Ex)\Psi(x)$ by $(Ex)(P(x) \& \Psi(x))$; and then those sentences are defined as valid which are derivable from the sentences $\Phi^{(P)}$ correlated with sentences Φ which are valid in T . The theory thus resulting is called $T^{(P)}$ (' T relativized to P '). In the case where T is axiomatisable there is a standard process of passing from an axiom system of T to an axiom system of $T^{(P)}$; if the axiom system of T is finite and there are only finitely many functors and individual symbols in T , then the corresponding axiom system of $T^{(P)}$ is also finite.

Still another near relatedness of two theories occurs : that between a theory T and an 'inessential extension' of it, obtained by adding to T finitely or denumerably many individual constants, but without further axioms. A theory and an inessential extension of it behave alike with respect to undecidability as well as to essential undecidability.

Now the statements on which the indirect method relies are the following

(1) If a theory T_1 is interpretable in a consistent theory T_2 or in an inessential extension of it then, if T_1 is essentially undecidable, the same holds for T_2 ; and if T_1 has a finitely axiomatisable and essentially undecidable sub-theory, the same holds for T_2 .

(2) If T_1 is an essentially undecidable and finitely axiomatisable theory which is weakly interpretable in T_2 or in an inessential extension of T_2 , then T_2 and every sub-theory of T_2 with the same constants is undecidable, and by adding finitely many suitably chosen axioms to T_2 an essentially undecidable theory is effectively obtainable.

(3) A theory $T^{(P)}$ obtained from a theory T by relativizing to a unary predicator P (not in T) behaves like T with respect to essential undecidability (though not generally with respect to mere undecidability).

From these theorems applications are made, in three ways, to proofs of undecidability. All these proofs are based on the essential undecidability of the finitely axiomatisable theory Q . Like Q also, by (3), the theory $Q^{(N)}$ (Q relativised to N) is finitely axiomatisable and essentially undecidable.

Let J be the theory whose non-logical constants are the functors $+$ and $\times$ and whose valid sentences are those which hold with respect to the domain of integers by the usual interpretation of the functors. Then J is consistent and complete. Further $Q^{(N)}$ is interpretable in J . In fact O , Sy and N can

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be defined in J ; in particular $N(x)$ —(x being a natural number)—can be defined as the condition that x is a sum of four squares. Hence, by (2), J and each of its sub-theories with the same constants is undecidable. Among the sub-theories of J which thus are found to be undecidable are the following finitely axiomatisable theories: the elementary theory of rings, and that of integral domains, both with or without the axiom of commutativity of the product, and with or without existence of a unit being postulated.

In an analogous way the undecidability of the elementary theory of ordered rings or integral domains (with the corresponding alternative conditions) can be proved. For this it is sufficient to consider, instead of J , the theory $J^<$, to be got from J by adding the constant $<$ with the usual interpretation.

It is to be observed that all the said axiomatisable theories of rings are only undecidable but not essentially undecidable. In fact they all are sub-theories of the elementary theory of real numbers (in which the schema of the Dedekind continuity axiom is applicable only to predicates in standard form); and this theory was shown by Tarski to be decidable.

Essential undecidability is proved of the theory of non-densely ordered rings, which differs from the theory of ordered rings by the addition of one axiom excluding density. The proof goes by directly interpreting $Q^{(N)}$ in this theory and then applying theorem 1.

Finally, in Part III, the theorems 1-3 are applied for proving the undecidability of elementary group theory, *i.e.* the theory G whose only non-logical constant is the binary functor $\circ$, and whose valid sentences are those derivable from the axioms $(x)(y)(z)(x \circ (y \circ z) = (x \circ y) \circ z)$, $(x)(y)(\exists z)(x = y \circ z)$, $(x)(y)(\exists z)(x = z \circ y)$.

The proof goes by the following steps. First it is shown that J is interpretable in the theory J^+ whose non-logical constants are the individual symbol 1, the functor $+$ and the binary predicate x/y , and whose valid sentences are those which hold by the interpretation of x/y as (x) is divisor of (y), and the usual interpretation of 1 and $+$. Of course J^+ is consistent and complete.

Since $Q^{(N)}$, as already stated, is interpretable in J , it follows that it is also interpretable in J^+ ; hence, by 1, there exists a finitely axiomatisable and essentially undecidable sub-theory of J^+ (which can be effectively constructed). From J^+ by relativising the theory $J^{+(G)}$ is obtained. Now $J^{+(G)}$ is proved to be weakly interpretable in the theory G' , which is obtained from G by merely adding an individual symbol c and thus is an inessential extension of G .

For this purpose a theory G'' is considered which has the same constants as G' and whose valid formulae are those which hold with respect to the domain of permutations of the integers upon the following interpretation of the constants: c is the permutation mapping each integer k into $k + 1$, and $\circ$ is the composition of permutations. Obviously G'' is consistent and the

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axioms of G' are valid in G'' , so that G' is a sub-theory of G'' . If now the constants of $J^{+(C)}$ are added to G'' with the defining axioms

$$(x)(C(x) \longleftrightarrow x \circ c = c \circ x), \quad (x)(x = 1 \longleftrightarrow x = c),$$

$$(x)(y)(z)(x = y + z \longleftrightarrow x = y \circ z),$$

$$(x)(y)(x/y \longleftrightarrow x \circ c = c \circ x \ \& \ (z)(x \circ z = z \circ x \rightarrow y \circ z = z \circ y)),$$

then in the resulting theory, which like G'' is consistent, every valid sentence of $J^{+(C)}$ is again valid. Namely every valid sentence of $J^{+(C)}$ is derivable from sentences $\Phi_i^{(C)}$ ($i = 1, \dots, r$) correlated to valid sentences Φ_i of J^+ . But for each of these sentences $\Phi_i^{(C)}$ by means of the axioms a biimplication $\Phi_i^{(C)} \longleftrightarrow \Psi_i$ is derivable, where Ψ_i is a sentence of G'' which—as a nearer discussion shows—is valid in G'' if the formula $\Psi_i^{(C)}$ holds with respect to the domain of permutations of the integers upon the following interpretations of the constants: $C(x)$ is the property of being a power of the permutation c mapping k into $k + 1$, 1 is c , $+$ is the same as $\circ$, and x/y holds if there are integers m, n such that $x = c^m$, $y = c^n$ and m/n . From this, however, it follows that Ψ_i holds in G'' if Φ_i holds in J^+ , which indeed has been assumed.

So it follows that $J^{+(C)}$ is interpretable in G'' and hence weakly interpretable in G' . Therefore by theorem (2), G as well as every sub-theory of G with the same constants is undecidable; further we can add finitely many axioms to G , without a new constant, in such a way that the resulting theory is essentially undecidable.

G itself is not essentially undecidable. In fact Wanda Szmielew proved that the theory we get from G by adding the commutative law to the axioms is decidable.

As Tarski expressly remarks, the undecidability of G does not imply the unsolvability of the Dehn word problem. This problem is not a question of undecidability of a theory in the sense here considered, but can be reduced to that about undecidability of the subset of those valid sentences of G in which all the quantifiers are general quantifiers extending over the whole sentence. A solution of this problem with the result of undecidability has in the meantime been published by P. S. Novikov. Some further applications of the methods described, concerning elementary group theory are mentioned at the end.

On the whole the methods explained in the monograph prove to be very powerful. By handling the theories here described a new fruitful branch of axiomatical investigation is brought about, which yields many unexpected insights in the structure of mathematics. The presentation of this new world of concepts is very instructive and stimulating. It was a lucky idea of the editors of 'Studies in Logic' to suggest to the authors the publication of the subject as a monograph. The book is dedicated to Heinrich Scholz.

PAUL BERNAYS

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L'Interprétation physique de la mécanique ondulatoire et des théories quantiques.

By P. Février.

Gautier-Villars, Paris, 1956. Pp. vii + 216. 3200 fr.

TODAY the interpretation of the formalism of quantum mechanics is again being criticised. One reason for it is that established theory cannot deal with high energy phenomena and with unstable particles like mesons. Another reason is that this failure has aroused the metaphysical desires of some physicists who now propose to turn back the clock and to re-introduce determinism. Mme Février discusses here the types of interpretation that may be imposed on quantum mechanics. The outcome of her argument is, I think, convincing. No interpretation that can possibly be imagined can ever bring back classical causality. Quantum mechanics is, and remains, a statistical theory.

This is of course not a new conclusion, and the vast majority of physicists would concur in it. Moreover, Mme Février's approach is close to that of von Neumann and Birkhoff. Her arguments, however, are more detailed; and she makes use of ideas of interest to the methodologist (which were first proposed by M. Destouches and the author, see this *Journal*, 1954, 256-261).

Scientific method is concerned with constructing theories rather than with inventing isolated hypotheses. A theory must possess a domain of adequacy, that is, there exists a set of experiments the theory is capable of describing. Although no theory is completely adequate or perfect, we must make sure that the results of future measurements which are predicted fall into the domain. Only then are the results relevant to the theory and, therefore, capable of testing it. Physics develops by constructing theories which have an increasing domain of adequacy. The domain of a new theory encloses that of the old theory, and this entails that part of the formal structure is taken over from the old to the new theory. This condition expresses Bohr's Correspondence Principle in a more general form. (see E. H. Hutten, *Proc. Aristotelian Soc.*, 1949, 49, pp. 115 ff.).

Another requirement that a theory has to satisfy is that it must be capable of predicting the results of future measurements when certain initial data are given. More often than not the prediction is uncertain, that is, involves probabilities. A general method of prediction is this: the original data, e.g. of position and momentum of a particle, are first transcribed in a formal language by making them correspond to 'initial elements' of an abstract ensemble, i.e. of the wave function. Then the initial elements are changed into other abstract elements for the time for which the prediction is to hold, i.e. the wave function is evaluated for a later time. Finally the predicted values are found by a time-independent law that expresses a generalised process of spectral decomposition, i.e. a Fourier analysis.

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This 'theory of prediction' is obviously based on the method by which the Schrödinger equation is solved. According to Mme F  vrier, the predictive characteristics of physical theories divide them into two, mutually exclusive, classes, viz. 'objectivist' and 'subjectivist'. In an objectivist theory, e.g. of classical physics, there exist state variables which allow us to calculate all other quantities. The state variables are simultaneously measurable, in principle; the theories are determinist, etc. We need consider only the state of the system, and the process of measurement or the influence of the observer is of no account. The subjectivist theories do not possess state variables; there exists at least one pair of variables that cannot be measured simultaneously, both in principle and in fact; the theories are indeterminist; we have complementarity, etc.

Another characteristic of theories is their completeness. A theory Th_1 is more complete than another Th_0 if it contains a parameter which the latter ignores, provided Th_1 predicts the same results as Th_0 in the domain of adequacy of Th_0 when the same initial data are used. This entails that, if two parameters are not, in principle, simultaneously measurable in Th_0 , they are equally not so measurable in the more complete theory Th_1 . Thus wave mechanics turns out to be an open or incomplete theory. We can always suppose that it leaves out certain parameters which a more complete theory would include: but such a theory has the same structure as the original one. It is therefore impossible to re-establish determinism by trying to complete quantum mechanics. No microphysical theory can be constructed that is deterministic and of such a kind that all its parameters are in principle (and without restriction) measurable.

Einstein was therefore quite right when he declared that the actual quantum theories are incomplete, in the sense that it is always possible to introduce more parameters: but any more complete theory has the same 'subjectivist' structure. No appeal to 'ignored' parameters can help, for a deterministic theory can never deal adequately with quantum phenomena. Quantum mechanics is an essentially incomplete theory.

This characteristic of quantum mechanics is shown up more clearly when we investigate how quantum phenomena are measured. The author begins the discussion with a philosophical question. Should we be 'realists' and assume *a priori* the existence of the physical world, that is, before we have measured anything? Or should we be 'phenomenalists' accepting as real only what has been observed and measured? This well-known philosophical puzzle has been posed only too often since quantum mechanics began. Mme F  vrier rightly says that so-called realist descriptions make use of 'hidden' parameters which are in principle inaccessible to measurement. In this manner they succeed in describing an 'objective' world in which everything happens according to deterministic laws, independently of observer and of measuring instrument. But such a description, though more

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complete, merely adds fictitious, i.e. not measurable, quantities: it is pure metaphysics. While recognising that both phenomenism and realism are philosophical theories, Mme F  vrier feels that phenomenalist interpretations are the lesser of two evils. Her ideal is expressed by Duhem's statement that a scientific theory should be free of all philosophic intent.

Thus the author prefers to oppose a 'predictive' to a 'realist' interpretation, because it is restricted to observable phenomena. If we want to calculate future states of a microphysical system, then we have to make use of a measuring instrument; for a microphysical phenomenon cannot be directly observed. The influence of the instrument cannot in principle be eliminated, and so there are always parameters that cannot be simultaneously measured with others. We can make predictions only at the cost of uncertainty. It is impossible to arrive at predictions with the help of a 'hidden' determinism, and so realist descriptions are not refutable by experiment and, therefore, metaphysical.

Mme F  vrier also discusses the idea of a pilot-wave which de Broglie had renounced at the Solvay Congress in 1927 in favour of Born's statistical interpretation. The pilot-wave as well as the theory of the double solution has recently been put forward again by Bohm and Vigier and also by de Broglie himself. But neither proposal is successful in bringing back determinism. There cannot be a functional relation between a physical wave which may guide a particle and the abstract Schr  dinger wave which allows us to predict the future state of the particle. The waves are only statistically correlated to one another.

Empirical propositions are about the initial conditions or data from which we start in order to predict the results of later measurements. Or, by means of logic we can derive from the original propositions new ones that are equally concerned with experiment. Such a logical calculus of experimental propositions differs in structure for the two types of physical theory. In an objectivist or classical theory the propositions follow a Boolean algebra. Since a quantum theory contains variables not simultaneously measurable, a proposition such as p & q is excluded and, with it, the possibility of using Boolean algebra. A modal logic may be used instead.

As I have said before, the conclusions reached by Mme F  vrier are not new, e.g. London and Bauer, von Neumann and Birkhoff, or Reichenbach among others more or less arrived at them as well. The arguments presented in this book are, however, more detailed and, though sometimes difficult to follow, logically rigorous and elegant. Anyone who is out of sympathy with the reactionary idea so fashionable today, that is, the idea of a 'hidden' determinism, will find here strong support, while the fellow traveller will—I hope—change his views if he reads this book.

E. H. HUTTEN

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Fact, Fiction, and Forecast. By N. Goodman.

University of London, The Athlone Press, 1954. Pp. 126.

THIS short but engaging book is made up of four chapters. Chapter I is a reprint of the article published by Professor Goodman in the *Journal of Philosophy*, 1947, 44, as 'The Problem of Counterfactual Conditionals'. Chapters II-IV contain three lectures which the author delivered in the University of London in 1953 under the general title *Fact, Fiction, and Forecast*.

The main theme of the book takes origin in the problem of possibility but it is developed along the lines which involve the treatment of dispositional properties, the validity of induction, confirmation theory, and the nature of scientific laws and lawlike hypotheses.

The analysis of counterfactual conditionals occupies the pivotal position in Goodman's enquiry. He believes that a solution to the problem of counterfactuals would give us the answer to a number of baffling questions in which the philosophy of science and the theory of knowledge abound. Now, the problem of counterfactuals, as Goodman sees it, consists in defining the circumstances under which a given counterfactual holds while the opposing conditional with the contradictory consequent fails to hold (p. 14). It is obvious that counterfactual conditionals cannot be interpreted as truth-functional compounds, and Goodman is right in emphasising that the truth-value of a counterfactual depends on a different kind of connection between the two component statements. The difficulty is to find a satisfactory explanation of this connection. In the first place certain circumstances not stated in the antecedent are assumed to occur. Secondly, the connection between the antecedent and the consequent is not a law of logic but what we call a natural, or physical, or causal law (p. 17). Thus we have the problem of 'relevant conditions' and the problem of 'law'.

With most commendable thoroughness Goodman examines a number of attempts at determining the set of relevant conditions for a given counterfactual only to come to the conclusion that we are not yet in possession of the required criterion. As regards the problem of law, Goodman shows that it involves the question of how to define the circumstances under which a statement is acceptable independently of the determination of any given instance.

The troublesome puzzle of the counterfactual conditional is taken up again in Chapter II. Here Goodman attacks the problem by examining the nature of dispositional statements and dispositional predicates, which apparently present us with less difficulty. His aim is to explain dispositional predicates in terms of manifest predicates without departing from the principle that both types of predicate apply to *actual* things. To put it in other words, the question is how beginning with a manifest predicate like 'burns' we can project it to a wider range of actual things by defining a correlative predicate like 'inflammable'. It is evident from this example

that the problem of dispositional predicates and that of projecting manifest predicates to unknown cases is closely bound up with the problem of induction, to which Goodman turns in Chapter III.

The traditional difficulties associated with the logic of induction result from fruitless attempts to justify inductive inferences. Goodman argues that such attempts should be abandoned and that efforts should be made instead to formulate rules which would define the difference between valid and invalid inductive inferences and distinguish between confirmable and non-confirmable hypotheses. The problem of induction conceived in this fashion turns out to be part of the more general problem of *projection*.

The prospects of a theory of projection are discussed in Chapter IV. To begin with Goodman explains what he means by actually projected hypotheses. In accordance with his definition a hypothesis is actually projected if and only if it is adopted and if at the time in question it has some undetermined instances, some positive instances, and no negative instances. A hypothesis which is exhausted, unsupported, or violated does not qualify for the status of an actually projected hypothesis (pp. 91 f.). On the basis of actual projection Goodman proceeds with the preliminaries which in his opinion may lead to a complete definition of projectibility. His initial step amounts to establishing three principles for eliminating unprojectible hypotheses: (i) a projected hypothesis H is to be rejected if it conflicts with a non-projected hypothesis K which is supported unviolated and unexhausted, and which has a no-less-well *entrenched* antecedent-predicate and a much better entrenched consequent-predicate; (ii) a projected hypothesis H with an ill entrenched consequent-predicate is to be rejected if it conflicts with another hypothesis K (1) that has the same antecedent-predicate and a much better entrenched consequent-predicate and (2) that is either (a) both supported and violated or (b) neither; (iii) a projected hypothesis H is to be rejected if there is another supported unviolated and unexhausted hypothesis K with the same consequent-predicate and a better entrenched antecedent-predicate, and if the supporting instances of H and K are the same but the antecedent-predicate of H applies to some things to which the antecedent-predicate of K does not apply.

Hypotheses which are supported unviolated and unexhausted, and which are unaffected by the principles of elimination, are called presumptively projectible hypotheses. As a further step in the development of the theory of projection the concept of initial projectibility index for presumptively projectible hypotheses is introduced and used for the purpose of defining degree of projectibility. A complete and final definition of projectibility is not worked out in the course of the present enquiry, which aims rather at illustrating some of the resources that a theory of projectibility may offer.

As a whole the book under review is both original and stimulating. Its chief merit seems to lie in what might be described as the clearing of the

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ground for a more promising treatment of some old but none the less fascinating problems. The topicality of the subjects discussed and the clarity of exposition are likely to earn the book many a reader from among methodologists, logicians, and philosophers.

CZESŁAW LEJEWSKI

The Dramatic Universe. Volume I: The Foundations of Natural Philosophy.

By J. G. Bennett.

Hodder and Stoughton, London, 1956. Pp. 534. 42s.

MR BENNETT is an author who is trying to give an all-embracing view of the cosmos. This introduces difficulties for his reviewer. One difficulty is that Volume I is therefore only half of a book, presenting half of the scheme, and Volume II is not expected to appear soon enough to justify postponing comment until the whole can be embraced. A much greater difficulty is that there are so many policemen about—specialist policemen on point duty, and semanticist policemen pounding the beat. The former insist on orderly movement according to certain rules along certain paths; the latter are likely to turn up anywhere. Their first question to the author 'What little game do you think you're playing?' will be followed by a demand either for his credentials or for proof concerning the truth of his statements. Most scientists can tell what they think they are doing (if not what language game they are playing), most artists cannot, and Mr Bennett does not quite fit into either class. Consequently it may not be fair even to evaluate his book in terms of what he modestly claims it to be: an attempt to show that it may one day be possible to present a philosophical system embracing all human experience.

This volume is about the processes studied by physics, chemistry, biology, and cosmology; the second volume is to be about values.

After some preliminary chapters on knowledge and language, entities are classified in terms of twelve levels of organisation. The first four cover sub-animate existence: (1) the undifferentiated ground-state, hyle; (2) polarity, invariant being (from the neutrino to the μ -meson); (3) relatedness, identical recurrence (nucleons, etc.); (4) subsistence, composite wholeness (atoms and molecules). After a transitional level (active surface, colloids) there are four levels of animate existence (viruses, cells, metazoa, species). Then a second transitional level (the biosphere) is followed by four levels of 'supra-animate' existence (planets, solar systems, galaxies, the knowable universe).

For describing process, four 'determining conditions', forming a six-dimensional framework, are used: space, time, eternity, and hyparxis. Space and time are the dimensions of the actual. Eternity is the dimension of potentiality, relevant to probability. Hyparxis is the measure of

ableness-to-be, recurrence in time; the hyparchic interval is said to vanish when recurrences of an entity are identical, and would thus seem to be a measure of development and decay on a given level of organisation.

One recognises the things that put the policemen on the alert—mention of the biosphere and eternity—and two warnings are enough. It had better be said at once that this book has nothing to offer those to whom 'speculative' is invariably a derogatory adjective or those who cannot enjoy an hypothesis before they have decided whether it is falsifiable or confirmable; it will be of no more use to them than is a handful of fertiliser to a hungry man, or a gymnasium to a sprinter in the course of a race. I suspect that this leaves a class of readers that at present is rather small.

It is concerning the first four levels of organisation that Mr Bennett really seems to have something to say. It is on these levels that the six-dimensional geometry appears to be invested with some quantitative meaning in relation to motion, energy, action, the mode of existence of elementary particles and their modes of interaction. The discussion is detailed, often ingenious.

The concept of a time-like dimension along which probabilities may be measured is attractive—but of course in the absence of any generally acceptable way of talking about probability, which seems to bring out the worst in the best of us, one's sales resistance to a new concept is rather low. The concept of the time-like dimension of hyparxis is difficult to grasp, but it may possibly provide a useful way of talking about certain types of process.

Concerning the other levels of organisation, Mr Bennett is relatively perfunctory. His levels of animate existence are the familiar ones. Here, the two time-like dimensions seem less relevant—but not entirely irrelevant. For example:

Sexipotent entities [cells] have no variation inherent in their eternal pattern and their potentialities must therefore be worked out through multiplication and recurrence. The sensitive hyparchic regulator has a space-like component, directed towards the environment, and it has also an eternal component, directed towards the common pattern. It thus provides a linkage between unity and multiplicity that is both flexible and restrictive. The restrictive character of the organic sensitivity in a single cell is due to the absence of mutual regulation between different levels of organisation.

The first three sentences appear to apply to the nucleus rather than to the cell; otherwise the implication is that cell differentiation occurs by jumps when cells divide, each jump being regarded as occurring after the independent life of the parent cell has ceased and before the independent lives of the new cells have commenced. The last sentence is quite unsatisfactory, unless I have misunderstood it: cells do regulate organisms, and vice versa.

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At the top of the animate levels, the author puts the biosphere, a disreputable concept. If this is regarded simply as the domain of ecological relations, there is nothing to get bothered about; it is the first reference to its rôle, that makes one feel uncomfortable. Photo-synthesis, yes; oxygen circulation, yes; but 'The Biosphere exists to transform energies required for the maintenance of some form of hypernomic existence probably within the solar system' gives one the feeling that Mr Bennett is here closing his eyes, holding his nose, and jumping into a language he has hitherto succeeded in avoiding. The brief section he devotes to 'the cosmic order', his last four 'supra-animate' levels, appears to be no more than a superficial account of the descriptive matter in any elementary textbook of astronomy, with his systematic terminology just stuck on. There seems to be nothing here that might suggest new concepts of process or relation. However drawn emotionally one may be towards any aesthetically satisfying principle of cosmology, there appears to be nothing here that cannot be talked about in the languages of the author's four lowest levels—with the possible exception of gravitation. After all, there is the simple fact that the emergence of what we call life came after the formation of the galaxies and after a lot of geological process as well.

It should be mentioned that few of the names in the index are not considered eminently reputable. Aristotle, Kant, Poincaré, Minkowski, Einstein, Whitehead (to whom I think the book owes most) and Eddington are among the most frequently referred to. However, the fact that there is reference also to Gurdjieff calls for a comment that is perhaps of wider relevance.

Gurdjieff practised a system of psychotherapy which, one may believe, has given relief or satisfaction, directly or indirectly, to several people: i.e., in certain circumstances, it works. The system's verbal form is allegory or myth, embracing the universe; its effect, like that of poetry or liturgy, is to work on people, to alter states of mind. Its language game is different from those used for other purposes, such as making predictions about the world of physics. It would appear, however, that Mr Bennett, Ouspensky, and others, impressed by the effect Gurdjieff's words had on their states of mind, proceeded to confuse anagogical efficacy with literal truth and to regard these words as expressing propositions about the world of physics and biology. It is interesting that they should have done so, as it seems unlikely that they would have regarded, say, the koans of Zen in the same way.

It seems fair therefore to regard Mr Bennett's introductory acknowledgment to Gurdjieff as equivalent to those of the form 'To . . ., without whose encouragement and support this book would never have been completed'. At very least, the book provides an advanced exercise in semantic analysis, and some readers may find it valuably provocative.

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The Direction of Time. By Hans Reichenbach.

University of California Press, Berkeley and Los Angeles, 1956. Pp.

vii + 286. £2 1s. 6d.

A PURELY mechanistic universe consisting of mass points which obey strictly deterministic laws presents various difficulties to the understanding, two of which are closely connected with the topic of the book under review. The first difficulty is that such a universe apparently does not provide any *physical* reason for the existence of a unidirected time. The second difficulty is that it apparently does not allow us to give an account of laws involving direction, such as the second law of thermodynamics. Both difficulties were solved, over fifty years ago, by Boltzmann's epoch-making investigations. The solution which Boltzmann proposes is very simple. According to Boltzmann the *second law* of thermodynamics is only statistically valid: given a class of closed physical systems, all of them in the same initial state of low entropy, the great majority of those systems will eventually assume a state of higher entropy, and this in spite of the fact that a *single system* behaves in a perfectly reversible way. The *direction of time* in a given part of the universe is determined by this behaviour of the majority and it is therefore a property which possesses neither spatial, nor temporal universality. (It may change from one epoch to the next, and it may also change from one part of the universe to the next.) Reichenbach's essay which is mainly concerned with what we above called the first difficulty may be regarded as an exposition, enriched and enlivened by many more or less relevant asides, of this solution of Boltzmann's (which Boltzmann himself managed to condense into two pages of vol. ii of his *Gastheorie*).

Its central part, i.e. the proof that time direction is a property not of a single system, but of an *ensemble* of systems with a common initial entropy, is based upon five assumptions which Reichenbach calls the *hypothesis of the branch structure* and which may be regarded as a slight clarification of Boltzmann's own presuppositions. It is interesting to note that three of these assumptions (3, 4, 5 on p. 136) have been proved to follow from classical mechanics (the proof is due to the work of Birkhoff and his school). In particular assumption 5 ('in the vast majority of branch systems the directions towards higher entropy are parallel to one another, and to that of the main system') can be shown to be a highly probable consequence of the laws of classical mechanics (it is valid for 'almost all' points of the trajectory of the 'universe'). This suggests that by replacing assumption 1 ('the entropy of the universe is at present low') with the assumption that the universe is dwelling in a state in which assumption 5 is strictly valid it would be possible to define a time direction for the whole of the universe. That such a procedure should be possible, at least for some stages of the development of the universe, is also supported by the fact, first made known by O. Heckmann,

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that a classical universe is unstable and must expand, or contract, or pulsate just as its relativistic colleagues.

Whereas Boltzmann was dealing with a mechanistic universe, Reichenbach assumes 'that the quantum physics of today is in need of Boltzmann's ideas just as much as the physics based upon Newton's mechanics for the very reason that this modern physics, too, did not discover irreversibility in its elementary processes' (134). This assertion touches a sore point of the interpretation of quantum theory. For few physicists would be prepared to admit that the continuous reversible changes of state implicit in Schrödinger's equation, are the only elementary processes of quantum mechanics. Both Bohr and von Neumann have asserted a dualism of reversible and irreversible processes, and Landé has even gone so far as altogether to deny that Schrödinger's equation may be regarded as a process equation. On the other hand it seems that attempts, such as Ludwig's, to explain the 'quantum jumps' as macroscopic phenomena which microscopically can be understood on the basis of Schrödinger's equation alone are either open to the reversibility objection, or employ a postulate which is equivalent with the assumption of irreversibility upon the macrolevel. This being the case the extension of Boltzmann's procedure to the domain of quantum mechanics seems certainly to be advisable.

The exposition of Boltzmann's theory is only part of the content of Reichenbach's essay. The remainder deals with such diverse topics as the emotive significance of time, entropy and information, quantum theory, the Feynman tracks and others. There are excellent suggestions (cf. the treatment of the Gibbs paradox on p. 63), long stretches of more or less efficient popularisation, repetitions of arguments and results which are well-known from other works of Reichenbach's. On reading the book one frequently (but not always) finds oneself confronted with the rather perplexing question as to whether that new discipline, called 'philosophy of science' is really as new as it looks, or whether it is only the old enterprise of popularisation made new and fashionable by the use, in addition to the technical terms of physics, of the technical terms of the one or another 'scientific' philosophy. But this question leads already far beyond the book under review, for it concerns nearly the whole of the present 'philosophy of science'. The fact that it does not apply to the whole of *Direction of Time* recommends this book both to scientists and to philosophers.

P. K. FEYERABEND

Experiment and Theory in Physics. By Max Born.

Dover Publications, New York, 1956. Pp. 44. 60c.

THIS short essay contains the substance of an address which the author delivered in 1943 to philosophers and scientists: In it Professor Born puts

forward some of his reflections on the nature of scientific inquiry taking as his starting point, on the one hand the extreme views of some distinguished experimentalists such as Lenard and Stark who reject all theory (as an invention of the Jews!) and on the other hand the views of some distinguished theoretical physicists, in particular Milne and Eddington, who held that the laws of nature could be discovered by epistemological and mathematical arguments without appeal to experiment.

The author's own views on the relation between experiment and theory are expressed mainly by means of illustrations from the history of physics, in particular the physics of the nineteenth and twentieth centuries, much of which has actually been given its present shape by Born himself. The highly exaggerated claims of the defenders of 'pure experiment without theory'—an expression which to most philosophers and physicists appears to be a contradiction in terms—are deflated by mentioning, among other things, Maxwell's most famous mathematical guess. This consisted in

adding without proper experimental reasons, the term $\frac{1}{c} \frac{\partial E}{\partial t}$ to the right-

hand side of the equation $\text{curl } H = 0$ —a modification which as Born emphasises leads to the electro-magnetic theory of light and to many other theoretical and experimental discoveries. Mathematical guessing of the kind described has nothing to do, however, with *a priori* reasoning; it is rather the work of an intellectual instinct in those who know the facts of experience and for whom mathematics is the natural language of physical inquiry.

The exaggerated claims of the *a priori* school of physics are attacked *via* a brief analysis of some of the works of Eddington and Milne. It is suggested that there are no good reasons for choosing on *a priori* grounds between these two *a priori* theories, even where their results are in a perfectly straightforward sense contradictory.

Born's own conception of the nature of a physical theory is very briefly explained by reference to the psychological *Gestalt*-theories. As these explain apprehension of shape in physical things, he wishes to explain apprehension of the invariants of physical theory and physical reality. Seeing a man behind a desk is not 'seeing coloured specks' and then 'composing them by a secondary process'. The creative physicist similarly sees a *Gestalt*; and he too may be right or wrong 'about the real shape of a partly known phenomenon'. If confirmed by experiment the physicist's apprehension produces new knowledge. But the ability to apprehend it rightly 'depends in a high degree on intuition and can hardly be learnt'.

The philosophical—and antiphilosophical—*obiter dicta* of physicists about their subject are often quite unenlightening to both philosophers and

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physicists, but Professor Born's lecture certainly does not belong to this type of literature. It is on the contrary highly interesting, informative, and in every way worthy of the attention of philosophers and, I should think, of physicists too.

S. KÖRNER

Descartes's Rules for the Direction of the Mind.

By Harold H. Joachim (Edit. Errol E. Harris).

George Allen & Unwin, 1957. Pp. 124. 10s. 6d.

THE late Professor Joachim's lectures on Descartes's *Rules* were originally delivered in the early 1930's as a supplement to the lectures later published as his *Logical Studies*. The manuscript of the Descartes lectures was, however, subsequently lost, and the volume under review consists of a reconstitution of these from the notes taken by the editor and by Professor John Austin when they were Joachim's pupils. It was obviously a labour of love, and it should be said at the outset that the work has been admirably done. This is the authentic voice of Joachim, his 'close and luminous' style, as it has been called. All those who have read Joachim's books, or who attended his lectures, would, I am sure, agree.

Whether, in the present philosophical climate, they would be able to accept Joachim's conclusions, is another matter. The purpose of the lectures seems to have been twofold. Joachim was, of course, concerned in the first place with giving a faithful, though critical, exposition of Descartes's view of how the reasoning process is properly directed; but he seems to have been equally concerned with confronting the Cartesian view with his own theory, usually known as the coherence theory of truth. The full force of this comparison and contrast is reserved for the last lecture, entitled the 'Philosophical Analysis of the Concrete'—a title which sufficiently indicates Joachim's line of criticism; but the double intention underlies the first three lectures as well, and tends to give Joachim's objections to the Cartesian view an air of prejudice.

The *Rules* were begun most probably soon after Descartes's return to Holland in the year 1628. The years of apprenticeship, of which he speaks in the Discourse, were at an end, and Descartes was prepared now to embark upon the mission that had been revealed to him on the famous night of 10th October 1619. The first nine months were spent, he tells us in a letter a few years later to Mersenne, in the composition of a little treatise on divinity—a first sketch, it may be supposed, of the subsequent *Meditations*. But the task on which Descartes had set his heart was the elaboration of a complete physical theory of the universe. This work, which Descartes

jestingly described as his 'Roman du Monde' was not destined to be published. The condemnation of Galileo for maintaining the movement of the earth, which was an integral part of Descartes's own conception of the universe, was enough to make him suspend publication of the work as a whole, and to let his views infiltrate, as it were, through two of the three treatises prefaced by the *Discourse*. The *Rules* were an attempt to codify the modes of reasoning which Descartes was putting into practice at the time. They reveal Descartes's constant preoccupation—the need to harmonise his physics and his metaphysics. It is a point to which Joachim paid no attention, any more than he seems to have been prepared to allow fully for the tentative and fragmentary character of the *Rules*. For that is what they are—tentative and fragmentary—and it is almost too easy to take note, as Joachim did in these lectures, of the many inconsistencies and unsafe conclusions. True, Descartes intended to write a book, of which the plan may be discerned in the *Rules*; but if the *Rules* may be regarded as a first sketch for the *Discourse* a sort of 'ballon d'essai', it was a balloon that never went up. Descartes left the *Rules* unfinished and unrevised except that he reduced them to four in the *Discourse*. The manuscript itself, as the editor shows, survived almost by chance.

This is not to say that the *Rules* may be neglected by students of Descartes. They are of great historical interest for the understanding of Descartes's finished works, the *Meditations* and the *Principles* as well as the *Discourse*, and it is a great pity that Joachim neglected this aspect of the work in favour of enforcing the contrast between his own theory and that of Descartes. The main charge brought against Descartes in these lectures is that his view of reason makes it largely an instrument of analysis and so incapable of grasping 'wholes'. But even wholeness is an aspect of the object which we perceive by analysis. In fact, we must take our reason, as T. S. Eliot once counselled us to take contemporary poetry, as we find it. We may describe it, if we please, as the 'false dissecting power', as long as we are aware that hard words break no bones. Even now, separated though we are by more than three centuries from the first formulation of the *cogito* in the *Discourse*, we can still, I think, understand Descartes's conception of the movement of the mind in the search for truth. The process begins with an initial intuition from which other intuitions flow by a rigorous deduction. Then comes an immense effort to transcend deduction itself by reintegrating its successive stages in a comprehensive vision of the whole body of human knowledge grounded on the evidence of its first truth and sharing in its light. An unattainable ideal, no doubt, and perhaps a misleading one, but far removed from the sort of rationalism which Joachim appeared to find in Descartes.

ARTHUR WOLLASTON

ANNUAL CONFERENCE OF THE PHILOSOPHY OF SCIENCE GROUP

THE third annual conference of the Philosophy of Science Group of the British Society for the History of Science was held from the 19th to the 21st September 1958 at St Hilda's College, Oxford, under the Chairmanship of Professor R. O. Kapp, chairman of the group, and was attended by about sixty members and guests. Dr M. B. Hesse was conference secretary.

Symposia were held on 'Simplicity in Scientific Theory', 'The Scientific Status of Economics', 'Other People's Concepts: Interpretations in Anthropology' and 'Theoretical Concepts'.

In the first symposium, papers were read by Dr H. R. Post and Dr Mary B. Hesse, and Professor R. O. Kapp took the chair. Regarding simplicity as a third requisite of a theory, alongside logical consistency and correspondence with experience, Dr Post suggested a case for attempting to define this quality, noting that it could be shifted between concepts and relations: any theory could be expressed in the form $X = 0$, at the expense of complicating X . Three fields of simplicity were in fact distinguishable: the linguistic, the semantic and the pragmatic (suggestive power). Dr Hesse did not regard simplicity as valuable in itself as a linguistic or semantic characteristic of a theory, but only as a pragmatic characteristic, namely that it makes for ease of apprehension. Linguistic simplicity, depending on choice of technical terms or of co-ordinate system, was irrelevant to the content of a theory. The discussion revealed two shortcomings of terminology: the term 'theory' is still applied rather indiscriminately to many types of statement including uniquely specific inference, generalised description, deductive system, and even to observational description; and the term 'simple' suggests 'easy' to some and 'economical' to others. After consideration of examples afforded by atomic, evolutionary, relativity and quantum theory, however, the session ended with at least a shared intuition, if not with a definition, of one sense in which the term 'simplicity' might be used in speaking of a scientific theory.

Mr W. C. Kneale took the chair in the second symposium. Professor T. W. Hutchison outlined the controversy among economists on whether it is possible to identify and to separate judgments of value from judgments of fact in integrating the latter into a system meriting the adjective 'scientific'. He argued that 'is' and 'ought' statements are always separable in principle, and that the attempt must be made to separate them in developing a disciplined universe of discourse. Dr A. W. Stonier accepted the logical distinction but argued that, in economic research, the raw data (especially verbal behaviour), however factual they may appear, tend to have human evaluation built-in; it might be possible to identify and to separate valuation components in the course of an investigation, but it seemed hopeless to try and choose only factual data at the outset. Several speakers referred to situations where fact and value judgment are clearly separable, extending the discussion to include other social studies and operations research (in which value judgments are controlled by being made explicit). The view was also expressed that perhaps a notion of 'Science' that excluded from its legitimate field any concept of the motivation of human behaviour was unduly restrictive.

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Mr J. W. N. Watkins took the chair for the third session. Mr E. A. Gellner considered a problem of anthropological method: what should be the attitude of the investigator of an exotic culture to verbal statements that do not appear at first to 'make sense'? The assumption of 'primitive mentality', of a radically different (and 'inferior') mode of thinking, has gone out of fashion. Current techniques of interpretation assume that sense, *good sense*, is to be made of any statement provided sufficient account is taken of behavioural consequences, of the context in which the statement was made, or of idiosyncrasy of the speaker. Unless used with caution, however, these useful techniques may invite false conclusions: they can be used to make sense of any utterance whatever, and they exclude certain types of interpretation known to be valid, as where a statement may be intended to influence behaviour by virtue of its very absurdity. Mr E. R. Leach accepted Mr Gellner's scruples, but considered that the safeguard lay in developing a wider view of possible contextual relations (including relations with the language as a whole, as in punning) rather than in rejecting the contextual approach. The conference came to grips with this problem by proceeding rapidly to generalise it to embrace the Freudian analysis of the *lapsus*, the distinction between expository and emotive uses of language, the peculiarities of abusive and amatory word-play, and the difficulties of conceptual analysis and interdisciplinary communication within one culture. Most speakers thought that there was little danger in the assumption that nearly all phenomena, including noise, are explicable in some sense, provided this is not held to entail that every phenomenon is worth explaining or that any statement (e.g. in a political doctrine) is *ipso facto* valid.

Dr A. C. Crombie took the chair for the fourth session. Mr G. Buchdahl made a case for distinguishing two types or levels of scientific discourse, the phenomenological and the epistemological, about theoretical entities such as atoms. He drew on the history of atomic theory in the first half of the nineteenth century for examples of how confusion of these two levels can lead to theoretical impasse: given certain phenomenological relations, there were phenomenological and epistemological questions about what unobservable things could have properties that would yield these observable relations. Dr J. Agassi held that there are meaningful connections between some views concerning the possibility of theoretical knowledge, some views concerning reality and some views concerning the method of learning. Discussion was vigorous, turning largely on whether using a concept that refers to a hypothetical entity entails any assumption about existence, and on the validity of attempts to progress from 'These phenomena occur *as if there existed* such and such entities having such and such properties' to 'These phenomena occur *because there exist* such and such entities, etc.'.

The conference closed with a general discussion of possible subjects for a fourth annual conference to be held in September 1959.

The liveliness and quality of discussion throughout the conference, and the success shown by workers from many different fields in 'tuning in' on one another's modes of thought, provided welcome assurance that the 'fragmentation of science' is not everywhere accepted as a necessary evil. The informality of the proceedings, moreover, permitted the rapid and fruitful exchange of nascent ideas, in which the historian, the philosopher, and the logician could mediate when any purposes threatened to become crossed.

G. E. DENYER

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Dialectica, 1957, II, Nos. 1/2

C. Perelman, 'Evidence et Preuve'

It is shown that a theory of proof which repudiates the classical conception of the obvious can nevertheless be elaborated, thanks to the elements provided by the analysis of reasoning, the study of which, unlike the study of demonstration, has for centuries been neglected.

W. Pauli, 'Phänomen und physikalische Realität'

Without accepting or favouring a particular philosophical '-ism', 'phenomenon' and 'reality' are analysed from the standpoint of the professional everyday life of physicists. The logical structures of physical theories, including its characteristic connections with observation and experimentation, is briefly indicated, with classical mechanics, classical relativistic field theories and quantum mechanics as examples. It is emphasised that physicists consider their science as being in the course of a development. The problem is therefore not whether or not the present theories will remain as they are, but merely in which direction will they change?

J. Rossel, 'Caractéristiques, tendances et implications de la recherche atomique actuelle'

The present situation of scientific activity, especially in atomic physics, has led to an appreciable alteration of the traditional spirit in basic research. The main reason for this modification appears to be the increasing bond between science and technology. As a consequence of this deep and intricate relationship, the influence of the political state, and of large financial organisations upon fundamental scientific work, has developed to a degree as yet unknown.

On the other hand, the result has been an industrialisation of basic research. From this state of affairs arises a limitation in the freedom of research and information, and often for the physicist a questionable propensity to megalomania.

The need for a study involving both science and philosophy is necessary when considering the implication of atomic research.

J.-L. Destouches, 'Aspect dialectique de la notion de système physique'

The essential part played in physics by the notion of system is discussed. We investigate here the latest *functional theory of particles* which consist in using for the representation of a particle a more complete picture than those which were used till now, the picture of a function u of a functional separable space.

H. König, 'Zur Analyse des Grössenbegriffs'

The author shows that many difficulties disappear when two entirely different axiomatic bases are accepted from which two entirely different manners of calculating with quantities and thinking in quantities are derived.

R. P. Dubarle, 'Formalisations et théorèmes critiques'

The writer aims at disengaging the philosophical significance of various mathematical theorems dealing with formalisations. These theorems form the part of

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epistemology which may be expounded in the technical form proper to that branch of science and bring together the elements of a self-critical reflection of mathematical thought. They also suggest that the act of mathematical thought should be understood as the unfurling of a thinking process implying simultaneously a deliberate initiative of thought, an intuitive field altogether indefinite and a logical normative requirement. In conclusion, they induce the philosophy of science to wonder whether or not mathematics as they stand are the only possible form of synthesis—in the strict sense of the term—between these three moments or factors of thought.

G. Bouligand, 'L'Activité mathématique et son dualisme'

A comprehensive study of mathematical thought, taking the actual work for its basis, will examine mathematical activity first. Such activity has always set itself mutually dependent aims of two sorts:

The solving of problems, i.e. determining some unknown element under precise conditions: and the achieving of a synthesis which, in the presence of new problems, simplifies and, if necessary, co-ordinates anew the known facts of whatever sort (methods, operational potentialities . . .).

This is what allows us to draw up a specific scheme regarding the historical evolution of deductive science, the dialectical aspect of that evolution has been stressed as far as possible. It brings in Logic in a natural way, and shows clearly how 'mathematisation' operates in a wide field.

H. Guggenheimer, 'Dialektische Synthese von Formalismus und Intuitionismus'

It is shown first that the cardinal and ordinal hierarchies of the theory of sets may be interpreted in a finitist, operationalist manner, without renouncing any of its fundamental features. This point of view has remarkable advantages as compared with a strictly axiomatic-formalistic interpretation. A way is indicated to overcome the problem of the inaccessible numbers.

D. Dugué, 'Quelques remarques sur le caractère provisoire de toute axiomatique'

Axiomatisation is given in this paper as the definition of a mathematical system with reference to all possible extensions. The set of these extensions may be infinite and so it is not impossible that the basis of any mathematical system is an infinite set of axioms.

This point of view is illustrated by examples from geometry. The author gives a statement of topology which he considers as an axiom boundary between certain mathematics and stochastic mathematics.

J. Ruesch, 'Principles of Human Communication'

Psychopathology and social pathology can be conceived as disturbed communication and the various methods of therapy, both physical and psychological, are geared to improving the organs and functions of communication in man.

Disturbed communication is considered as a special case of ordinary communication that is distorted through erroneous timing, deviations in intensity, and inappropriateness of messages.

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M. Monnier, 'Plan d'organisation, continuité et stades du développement des activités électriques cérébrales'

The analysis of the development of the electrical activity of the brain in children, with EEGs recorded in longitudinal series allows us to express certain general remarks on the organisation, continuity, and discontinuity (levels) of the brain development. The method is both objective and subjective.

The analysis of longitudinal series of EEGs shows that the electrical brain activity develops in a *continuous*, progressive way, according to an organisation plan (Bauplan). This plan is partially expressed in each EEG, but completed only in the definitely organised pattern of the 15 years old adolescent. In this continuity of the development, however, there appear certain periods when the organisation of the electrographic pattern seems to be intensified, accelerated, consolidated. These 'epochs', or levels, appear at 6 to 7 years, 9 to 9.6, 11, 13, and 15 years. It is possible to detect a certain correspondence between this conception of the development and that of Piaget and Inhelder. The chronology of the electro-physiological levels corresponds to that of the cognitive functions.

C. J. Herrick, 'Analytic and Integrative Nervous Functions'

Analytic functions, typified by reflexes, use stable inherited structures which are organised for adjustments in behavioural space and time. Individually acquired integrative functions, typified by conditioning, symbolism, and reasoning, are wholly internal and may use any nervous tissue not differentiated for other specific activities. They must be defined relativistically. The highest members of both classes are in the cerebral cortex, the first in the relatively stable projection centres, the second in the more labile areas of elaboration where there is no inflexible localisation of function. When consciousness emerges within the second class, the mechanism employed probably must be described in accordance with still unknown relativistic principles.

Charlotte Bühler, 'About the Goal-Structure of Human Life'

The present study is to survey some of the concepts and theoretical considerations to be applied in the study of individual's goal-setting, toward a general theory of goal-setting in human life.

W. Servais, 'Pédagogie mathématique ouverte'

As a philosophy, idoneism is most closely related to mathematics and its methods could and should exercise a deep influence over the teaching of mathematics.

Teaching should be an active dialectic of what is intuition, empirical, and rational. It should rely on concrete instances and progressively organise deductive inference, seeing to it that the requisite mental structures are acquired. Such elements would then mutually integrate, since the mathematical model would be disengaged, inferred, from the intuitive model.

Dialectica, 1957, II, Nos. 3/4

P. Bernays, 'Von der Syntax der Sprache zur Philosophie der Wissenschaften'

Newer trends of development in the schools of logical empirism are considered. It is shown how, by the correction of the original too simplifying theses of the Vienna school, one becomes reduced to the study of the traditional problems of the philosophy of science.

ABSTRACTS

A. C. Blanc, 'De l'emploi inadéquat du terme "primitif". Considérations sur l'évolution et la systématique'

The inadequate use of the term 'primitive' is due to a confusion between archaic primitivity proper, and secondary 'primitivity' or pseudo-primitivity. In any kind of evolution, the transitions from simplicity to complexity and from indistinctness to distinctness follow strictly similar courses. Three phases are recognisable: a pre-apogean phase of slow enrichment, an apogean phase of original polymorphism, and a post-apogean phase of segregation and specialisation. It is necessary to distinguish between:

1. the archaic simplicity of truly primitive forms and secondary simplicity of impoverished and specialised forms;
2. original polymorphism and secondary polymorphism due to hybridisation.

A. Mercier, 'Science et philosophie'

Science and philosophy are distinct, but they rely upon each other. Both are precarious, in different degrees; both are open.

There are two extreme ontological positions: A kind of scientism consisting in a radical positivism (there are no beings but limited beings), and the transcendental ontological position (the notion of being in the singular is meaningful as infinite source of all existence and as guarantee of the fact that there always will remain work to be done).

A dilemma arises: Neither of these positions can be proved to be the right one. Yet without the dilemma, it is doubtful whether any philosophical problem would arise at all.

There are strong reasons to think that the radical positivistic position is nearer to a closure, whereas the transcendental one warrants the openness of science as well as of philosophy.

R. Bayer, 'L'Évolution de l'intelligence et les formes modernes de la dialectique'

There are two prospects of intellectual evolution in the notion of dialectics: intelligence can either be the ultimate point of biological adaptation or it can already be the expression of reason. Within these open characteristics of scientific dialectics are to be found the interpretations studied here: Baldwin's pancalism, Binet's 'thought without image', Janet's and Piaget's interpretations which help to enrich the knowledge of the genesis of intelligence and its evolution.

L. Rougier, 'L'Évolution du concept de raison dans la pensée occidentale'

The word 'reason' acquired a meaning with the creation of deductive geometry by the Greeks of the fifth century B.C. These geometers substituted for the sensible evidence, which only establishes the 'what' of an observed fact, the intelligible evidence which explains the 'why' of such observed fact, by showing that it is the necessary result of a small number of statements, accepted as self-evident. Rising from the concrete to the abstract, the geometers isolated the intelligible essence from the sensible accident, and substituted for the real the possible. The empirical technics of the Orientals made place for the theoretical sciences of the Greeks: astronomy, mechanics, optics, and musical theory. Instead, however, of seeing mathematical concepts as creations of the mind, the Greek geometers considered them as

ABSTRACTS

pre-existing entities, discovered by the eyes of the soul. They furthermore disregarded the practical applications which alone can stimulate experimental sciences. There were the limitations and the causes of the eventual end of hellenic science.

The scholastics sought to reconcile reason with faith, by defining reason as the legacy of Greek science and philosophy, identified, after the thirteenth century, with the Aristotelian Encyclopedia. They failed in this endeavour. The doctrine of the 'double truth' was the admission of this failure.

In order to progress beyond the scholastic outlook, it was necessary to reject the precepts of Aristotle in the study of natural phenomena, to become free of the tyranny of theology, to develop a correct theory of knowledge, especially as regards the rôle of abstract ideas in the distinction between formal and empirical truths. It was necessary to interpret as a creation of the mind what the Greeks considered the discovery of pre-existing truths. The breakdown of the *synthetic a priori* of the Cartesians and the Kantians leads one to wonder what remains of the *a priori* requisites of reason. The last part of the article suggests an answer to this question.

C. Paris, 'Programme et position historique d'un rationalisme humaniste'

The critical attitude taken towards the idea of reason supplies the starting point; it is considered possible to overcome the resulting crisis by positing this idea anew, in order to establish a new rationalism which might be termed 'humanist'. That rationalism should first of all duly record the fact that our knowledge is strictly human and differs as such from an absolutely intellectual knowledge, in a sense which contrasts with the present rationalist outlook. The writer stresses the 'compositive' and 'divisive' aspects as well as the dynamic character of our rational activity, in the process of presenting the explicative possibilities of this dynamic understanding with reference to mathematics and natural science. Kantism is examined in its historical and programmatical position, stressing how deep the rationalist imprint is there, making it a blind alley as far as scientific dynamics are concerned. A way of overcoming this difficulty is discussed.

K. Reidemester, 'Spekulation und Vernunft'

A theory of knowledge (relating, e.g. to the exact sciences) can only exist as an independent philosophical discipline if we can establish a concept of scientific thought independently of an inquiry into individual sciences. This is the problem, which Wittgenstein tried to solve in his *Tractatus Logico Philosophicus*, not without touching on some very speculative and unknowable things. For that reason, we cannot understand the significance of the *Tractatus* without the idea of speculation. It is an essential and reasonable problem for the theory of knowledge to determine the nature and the limits of speculation. It is a relevant problem also for the theory of science, since physics and speculation have points of contact and some distinguished physicists have introduced speculative ideas uncritically into scientific exposition.

E. Dupréel, 'Consistance et valeurs'

The *degree of consistence* of an object results from its possibilities of resisting external attacks.

There are: the consistency of *beings*, the consistency of *notions* and the consistency of *values*. The most fundamental problems are those concerned with *hierarchy*

according to consistence, and above all those concerned with *promotion*, the passing to a higher grade (matter—life—knowledge—spirituality).

K. R. Popper, 'Probability, Magic, or Knowledge out of Ignorance'

A formal axiomatic calculus can be constructed comprising all the well-known laws of probability theory. This calculus can be interpreted in various ways. The present paper is a *criticism of the subjective interpretation*; that is to say, of any interpretation which assumes that probability expresses *degrees of incomplete knowledge*. It is shown in the paper that the subjective theory cannot lead to results which are compatible with the objective theory. This is due to various reasons, the most important of which is that the objective theory interprets '*b*' in '*p* (*a*, *b*)' as a statement of the *objective conditions* of an experiment and '*a*' as one of its possible results. The subjective theory on the other hand interprets '*b*' as our total relevant knowledge which will in general include some knowledge of previous results of the experiment. It is shown that this must lead to incompatibility owing to the fact that this knowledge of previous results must influence the value of the probability.

It is shown, in this way, that any probabilistic theory of the process of learning from experience—that is to say, any probabilistic theory of induction—must lead to contradictions.

F. Moch, 'Réflexions sur les probabilités'

Probability shows paradoxical characteristics in classical physics; although dependent upon a state of knowledge, and yielding information only as the total result of numerous trials, it seems nevertheless to be definable as an objective characteristic of the isolated event. When used by micro-physics, it breaks the unity of the physico-mathematical foundations which became apparent in bivalent logic, in its conception as a theory of any object, any event or any theory.

Analysis of these paradoxical characteristics and of the concepts 'object' and 'event' shows physics and mathematics regain their fundamental unity if bivalent logic is replaced by the trivalent logic of synthesis (*Dialectica* 35/36) or the logic of attitudes, which generalises it (*Dialectica* 39).

P. Nolfi, 'Spieltheorie und Willensfreiheit'

The present paper shows how the most important problem of 'freedom of will' can be interpreted and clarified within the framework of the theory of games.

S. Gagnebin, 'Structure et substructure de la géométrie'

The author endeavours to set forth the significance and importance of two writers on geometry. G. Bouligand brings out the algebraic and logical structure of that science and presents an 'axiomatic' introducing both the notions of 'whole' and 'group of transformation'. A progressive classification of problems is thus elaborated according to the kinds of solution that suit them. F. Gonseth, in his far more comprehensive work, analyses the intuitive, experimental and theoretical aspects of what used to constitute the elementary in classical treatises, then states the researches raised up by Euclid's postulate and leading to the development of the axiomatic method. Thanks to the introduction of such ideas as 'schematic correspondence' and 'model', geometry appears as a rational science in progress using the 'axiomatic' as its criterion.

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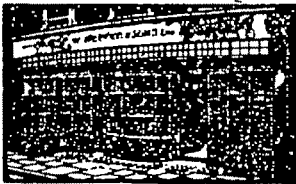
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